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OF THE

QUARTERLY JOURNAL,

No. XXXVIII.

We are requested by a Correspondent at Leeds to notice the following ERRATA in TAYLOR'S TABLES.

For 9.6446087 Tang. of 23° 48′ 19″, *read* 9.6445987

For 10.3553913 Cot. . . ditto . . *read* 10.3554013

TO OUR READERS AND CORRESPONDENTS.

A copious General Index to the first Twenty Volumes of this Journal will form the Twenty-first Volume.

We have acquiesced in the wish of our Correspondent at Geneva, under the intention of pursuing the subject.

Mr. Edwards's paper reached us sufficiently early, but having previously devoted much of the present Number of the *Journal* to similar subjects, we trust he will see the propriety of deferring its publication.

We are much obliged to PHILOCHEMICUS for his excellent remarks upon a recent Chemical publication: we reserve them to be embodied in our Review of the work, which will probably be ready for the next Number: it is under rigid examination.

The Queries of "A Mummy" may all be answered in the affirmative

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The Communications of Mr. Walsh, Dr. Sanders, and Mr. Burton; those of our old Correspondent O., of Dr. de Sanctis, and of a "Fellow of the Royal Society of Literature," have safely reached us, but are necessarily postponed.

We are not in the habit of giving opinions in the manner required of us in an anonymous letter from Liverpool.

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OF THE

QUARTERLY JOURNAL,

No. XXXIX.

ART. PAGE.

Erratum.—In Hutton's Tables, 8. Lond. 1822, P. 227, N. 75843; *for* 9255, *read* 9155.

TO OUR READERS AND CORRESPONDENTS.

We declined the insertion of the letter signed OMEGA, in consequence of the irrelevant matter which it contained; we shall be happy to receive and publish any temperate observations upon the subject, but they must not be mixed up with bridges and church steeples.

We are much obliged by the communications from Hull; but independently of the incorrectness of the drawing, we believe there are better means of attaining the same end.

The letters from Portsmouth are *anonymous*.

Q. E. D. may advantageously consult the Marquis of Worcester's "Century of Inventions."

We cannot at present interfere in the controversy alluded to by a "Member of the Astronomical Society;" at all events, we beg leave to postpone the publication of his communication till our ensuing number.

Z. is correct, as to the low expansive power of Platinum; it falls far short of that of the other metals; but its expense and specific gravity are *bars* to its application in the way he suggests.

If "A Member of the Mechanics' Institute" will be a little more explicit, we shall be happy to give him all the information in our power.

Several articles are necessarily postponed till our next Number —we shall then certainly review the work alluded to by Philo-Chemicus, but we cannot promise to adopt his suggestion.

THE

QUARTERLY JOURNAL.

April, 1825.

Art. I.—*Some Account of the Prangos Hay Plant of Northern India; prepared by permission of the Honourable Court of Directors of the East India Company.* By Mr. John Lindley, F.L.S., *&c. &c.*, Assistant Secretary at the Garden of the Horticultural Society of London.

[Communicated by the Author.]

In the north of India, in the neighbourhood of Imbal or Draz, grows a plant called Prangos; much employed as fodder for cattle, and of properties represented by the natives to be so marvellous, as to have excited doubts among the Europeans, whom reports of it had reached, as to its being more than an Oriental exaggeration. Owing to the little intercourse which takes place with the unfrequented districts where it was stated to grow, no opportunity occurred of gaining accurate information respecting it till the year 1822, when William Moorcroft, Esq., the superintendent of the Honourable East India Company's [illegible], on deputation to Upper Asia, having occasion to enter into communication with the Chinese authorities of Ela, undertook a journey to Draz, for the purpose of examining into the truth of the properties ascribed to the plant by the natives.

The information, thus acquired, appeared to this gentleman of such importance as to be worthy of an especial communication to the government at Fort William. Two chests of the seed, and

specimens of the Prangos Hay itself, were forwarded from India to this country, and presented by the Honourable Court of Directors to the Horticultural Society, with the correspondence between Mr. Moorcroft and the Indian government. Having had the honour to receive permission to use these important documents for the purpose of publication, I have prepared the following account of this remarkable plant, which may possibly become an object of great importance to our colonies in an agricultural point of view, whether we consider its amazing produce, its beneficial effects as a food for cattle, or the little care which is requisite in its cultivation.

The following are extracts from Mr. Moorcroft's letter, dated from Wakha, left bank of the Molbee Ches, 15th August, 1822:—

"The plant called Prangos is employed in the form of hay, as a winter fodder for sheep and goats, and frequently for neat cattle; but its seed, when eaten by horses, is said to produce inflammation of the eyes and temporary blindness. The properties of Prangos as a food appear to be heating, producing fatness in a space of time singularly short, and also to be destructive to the Fasciola Hepatica or Liver Fluke, which, in Britain, after a wet autumn, destroys some thousands of sheep by the rot, a disease that, to the best of my knowledge, has in its advanced stages hitherto proved incurable. The last-mentioned property of itself, if it be retained by the plant in Britain, and there appears no reason for suspecting that it will be lost, would render it especially valuable to our country. But this, taken along with its highly nutritious qualities, its vast yield, its easy culture, its great duration of life, its capability of flourishing on lands of the most inferior quality, and wholly unadapted to tillage, impart to it a general character of probable utility unrivalled in the history of agricultural productions. When once in possession of the ground, and for which the preparation is easy, it requires no subsequent ploughing, weeding, manuring, or other operation, save that of cutting, and of converting the foliage into hay. Of its duration I have two facts, *viz.*, one of its seeds having been carried westward along with those of Yellow Lucerne, above forty years ago,

and sown on the eastern frontier of Kashmeer, where they vegetated, and where the plants of the *first* growth still remain in a flourishing condition; in the second instance, the seeds were transported eastward, and sown upon rocks near Molbee, where their plants flourished for about forty years, but in consequence of a long period of drought, during which there fell scarcely either rain or snow, the Prangos perished along with the crops of that district in general.

" From various facts, it is conceived not unreasonable to presume that by the cultivation of this plant, moors and wastes, hitherto uncultivated, and a charge of disgrace to British agriculture, may be caused to produce large quantities of winter fodder, and that the yield of highlands and of downs, enjoying a considerable depth of soil, may be trebled. I have made every precautionary arrangement in my power by presents, &c., for gathering, drying, packing, and transpo ting a large quantity of the seed, and have left Mr. Guthrie, the apothecary, to superintend the operations. One cask will be transmitted through Kashmeer, and two others through Bushehar. And I take the liberty of submitting to the Most Noble the Governor-General in Council, the probability of this plant being of use to the new settlers, our countrymen, at the Cape of Good Hope, and to the colonists in general. As the Prangos has hitherto been of spontaneous growth alone, practices better adapted to the nature of the plant or of the country may be adopted at a future time; but from a view of its habitudes in its wild state, I venture to suggest that the seeds be dibbled singly into holes an inch deep and a foot apart, a short time before the rainy season.

" During three years the plants will be little productive, but in that interim they will not be in the way of any other surface crops."

Judging from the specimens sent by Mr. Moorcroft, each plant will produce about one and a half pound of dry fodder, which, allowing each plant to occupy four feet of ground when in perfection, will give a produce for bad land of more than a ton and three quarters each acre, which is nearly equal to the produce in

hay of the best English meadows. But if the distance recommended by Mr. Moorcroft be sufficient for the growth of the plants, that is to say, one foot, then allowing a plant to produce only half a pound of hay, an acre would yield the amazing weight of something more than nine tons and an half, a quantity which certainly appears to exceed credibility.

It is much to be regretted that from the length of time which elapsed between the despatch of the seeds by Mr. Moorcroft and their arrival in England, that is to say, from the 15th of August 1822, to the month of April, 1824, their vegetative powers had become so much exhausted as to render it extremely doubtful whether success will attend the experiments upon growing them. Now, however, that attention is called to the plant, other and speedier means may be employed for despatching the seed; no difficulty in procuring which can now be anticipated, Mr. Moorcroft having made arrangements with Ripghias, the Kenphun, and Mahomed Khan, the Chummul of Draz, for a supply of any required quantity of the seed.

The Prangos Hay Plant is a perennial herbaceous plant, with a large fleshy root-stock, usually measuring at the top from 18 to 22 inches in circumference, and formed by the aggregation of an infinite number of crowns or winter buds clustered together at or above the surface of the ground. The *crowns* are closely covered over by the coarse fibrous remains of the old leaves, by which the buds must be effectually protected from frost or accidents when the plant is in a state of rest. From each crown rises an abundance of finely cut leaves about two feet long, when dried, of a highly fragrant smell, extremely similar to that of very good new clover hay. They are supra-decompound, quite smooth, with linear, entire, or three-parted segments; their principal petiole is slightly sheathing at the base with a crisp thin margin; upwards it is solid, round, or slightly angular, with a smooth finely-striated skin. Of the secondary petioles there are from six to ten opposite pairs, according to the vigour of the leaf; they are in all respects like the primary petiole, except being smaller and more compressed, and having the first pair of their segments proceeding

from their very base. In these leaves the whole crop may be said to consist.

From the centre of the leaves rises the *flower-stem*, which I have only seen in a young and mutilated state. Good specimens of the *inflorescence* have not reached me; but from some imperfect umbels of flowers, I can state that the male and female flowers are produced upon distinct umbels. Of the *male flowers* the *umbels* are compound, shorter than the bracteæ by which they are subtended, and both axillary and terminal; the *bracteæ* are finely and deeply pinnatifid with three-parted segments, of which the end-lobe is broader than the rest, and often three-toothed. The *involucres* are both general and partial, each consisting of five or six membranous ovate-acuminate leaflets, which are shorter than the stalks of the umbellules, or of the florets. At the base of the umbel are clustered several scarious rudiments of florets. The *calyx* consists of five distinct ovate minute sepals. The *petals* are five, lanceolate, spreading, incurved, with a minute dorsal nerve. The *stamens* are five, spreading, the same length as the petals, and inserted opposite the sepals beneath a large, fleshy, slightly wavy *discus*, which surrounds two little processes, the rudiments of as many styles. The *filaments* are incurved, and quite smooth. The *anthers* large, square, innate, bilocular; each cell opening longitudinally with two valves. The *female flowers* have not yet been observed. The *fruit* is inferior, and consists of two united achenia, at maturity separating from base to summit from their common axis; it is oval-lanceolate, compressed, eight or nine lines long, and is crowned with two recurved styles, arising from the centre of a large, fleshy, wavy discus, and with the corky sepals of the persistent calyx. Of these achenia, the commissure or point of union is nearly flat, and narrower than their transverse diameter. Of each the pericarpium is corky, with five primary juga or elevations, which are in the centre produced into a corky wavy wing, and on each side covered densely with coarse tubercles; there are no secondary juga, and the valleculæ, or intervals, are concave and smooth. The *seed* is of the same form as the pericarpium, from which it is easily separable; it is covered

over with an indefinite number of colourless vittæ, both on the commissure and back; it has an involute horny albumen, and a minute, inverted, white embryo at its upper extremity; the cotyledons are flat and oval, the radicle rounded, and as long as the cotyledons.

From the foregoing description, which has been formed from such materials as have reached this country, it appears that the Prangos Hay Plant belongs to the natural order of Umbelliferæ, and that it bears much affinity to the genus *Cachrys*, with which it agrees in the corky nature of its pericarpium, in the absence of secondary juga, in having no vittæ, and in the involute structure of its albumen. With *Krubera* of Hoffmann, which it resembles in the general appearance of its fruit, it may also be compared, notwithstanding its difference of habit; with that genus, however, it cannot be united, on account of its involute not solid albumen, numerous vittæ, and lanceolate not emarginate petals. From *Laserpitium* it differs materially in having involute albumen, an indefinite number of vittæ, and no secondary juga, while its primary juga, which in Laserpitium are obsolete, are in Prangos the most conspicuous part of the fruit. To *Rumia* of Hoffmann it is not referable because of its solid pericarpium, distinct winged juga, and long flat achenia.

To revert, therefore, to Cachrys, with which, as I have already stated, Prangos has many points in common: if Cachrys Morisoni, the fruit of which has a solid, corky smooth pericarp, with its juga nearly obsolete, is to be considered the species in which the essential character of the genus is to be sought, it is obvious that Prangos cannot be considered of the same genus. But if Cachrys be admitted in the form under which it has been placed by Sprengel in the sixth volume of Römer and Schultes's Species Plantarum, it is equally certain that the subject of this article cannot be separated from it. Differences in the fruit and petals of Umbelliferæ are, however, by the common consent of botanists, admitted to be of such importance in fixing the characters of the genera of that order, that a combination of plants, like that which has been attempted in the work above quoted,

must be considered utterly subversive of analytical division, and can only lead to a return to the genera of Umbelliferæ as Linnæus left them.

Besides Krubera and Rumia, the distinctions between which and Prangos I have already explained, there is a third genus included in Cachrys by Sprengel, but separated by Professor Link, under the Bauhin's name of *Hippomarathrum*. From this Prangos seems principally to differ, in having entire, not pinnatifid, involucra, the juga winged, not rounded, and the petals lanceolate, not round with a broad involute segment; all points of great importance in characterizing umbelliferous plants.

Having thus shewn that the Prangos Hay Plant is strictly referable to no genus of Umbelliferæ at present constituted, I propose here to establish it with the following name and character:—

PRANGOS.

Char. Nat.—Calyx quinquedentatus. Petala æqualia, lanceolata, incurva, integerrima. Discus carnosus, crispus. Acnenia a dorso compressa. Pericarpium suberosum: commissura plana, angusta; jugis quinque primariis alatis, secundariis nullis. Semen multivittatum. Albumen involutum.—Herbæ *Asiæ temperatæ*. Involucra *universalia et partialia simplicia, polyphylla*. Flores *abortu monoici, lutei?* Folia *supradecomposita*.

Among the plants placed by Sprengel under his genus Cachrys, is the *Laserpitium ferulaceum* of Linnæus, found in the Crimea, a climate not very different from that of Draz, and described by Marschall von Bieberstein under the name of *Cachrys alata*. This having a winged corky fruit, like that of Prangos, and otherwise agreeing with it in character, the genus now established will consist of two species, which may be distinguished thus:—

1. Prangos *pabularia*. Mihi supra.
   P. foliis glabris.
2. Prangos *ferulacea*.
   P. foliis hirtis.
   Syn.—Cachrys orientalis ferulæ folio. *Tourn. it.* 2. p. 286 c. ic.
   Laserpitium ferulaceum. *Linn. Sp. Pl.* 358.
   Cachrys alata. *Bieb. Taur. Cauc.* I. 217.

ART. II.—*Some Observations on the Physiology of Speech, read to a Literary Society, at Nottingham, on January the 3rd*, 1825; by Marshall Hall, M.D., F.R.S.E., *&c. &c.*

[Communicated by the Author.]

THE different parts which compose the mouth and nostrils constitute the organ of *Speech*, as distinguished from the *Voice*, which is formed in the larynx, or upper part of the windpipe; and speech may be defined as consisting in those modifications of the vocal sounds, produced by impressions made upon the air, *after* it has left the organ of voice. These impressions are chiefly effected in the mouth; the office of the nostrils is far more simple, being merely to admit the egress of the air in the articulation of certain letters, whilst it is intercepted in that of others.

The parts of the mouth which principally conspire to perform the functions of articulation, are the lips, the teeth, the tongue, the palate, and the *velum pendulum palati*, or pendulous vail of the palate. The offices of each of these parts will be explained as we proceed, and, with the exception of that of the part last mentioned, will be readily understood by every one. Of this last part, it is only necessary to say, that it forms a sort of curtained arch, hanging over the posterior part of the tongue, and may be readily seen, on looking into the throat. The function of the pendulous vail of the palate is, in the enunciation of certain letters, to close the nostrils at their posterior orifices; for this purpose, it is drawn upwards, and accurately applied to the orifices into the nostrils, by means of an appropriate set of muscles.

In the course of the subsequent remarks, we shall have abundant occasion to notice the wonderful manner in which the different parts of the mouth concur to perform their very varied functions, of which articulation is only one. It may not be irlevant, indeed, very shortly to notice some other of the functions of these parts, before I proceed to the more immediate object of this essay. We shall thus be still more struck with admiration, that functions so very different should be performed

by the very same parts; and we shall thus be better enabled to deduce the functions of some of these parts, or to confirm the conclusions we may draw, relative to the nature of these functions, as obtaining in the mechanism of speech.

I would then, in the first place, refer to the function of deglutition, or swallowing.—In order to effect deglutition, every cavity or canal with which the mouth communicates must be accurately closed, with the exception of the pharynx and gullet, along which the substance swallowed is propelled, whilst the various muscles by which deglutition is effected are called into action:—the mouth is thus closed in front by the lips, the windpipe is closed and accurately secured by a little valve-like body, called the epiglottis; and the posterior openings of the nostrils are closed by the pendulous vail of the palate being drawn upwards and accurately applied to them. Thus is the cavity of the mouth completely shut, and excluded from its communications with the open air, or with the lungs. And if, from laughing or inadvertency, or from a defective action of the parts, as in palsy, any one of these functions be not adequately performed, the consequences are well known; the substances which ought to have passed down the pharynx and œsophagus, escape out of the mouth; or into the windpipe, inducing the most distressing coughing; or in some instances, though more rarely, into the nostrils.

I notice, in the second place, a function of the mouth equally different from deglutition and articulation. It is performed in the act of sucking, and in that of blowing the blow-pipe. The first of these acts implies a diminished, the second an increased, elasticity and pressure, with regard to the air contained in the mouth; and yet, during the continuance of these acts, we are enabled to breathe freely and uninterruptedly through the nostrils, whilst the cavity of the mouth is complete, and its posterior aperture closed by means of the pendulous vail of the palate, the windpipe and nostrils communicate freely behind that vail.

In the act of deglutition, then, the nostrils and the windpipe are accurately closed by their respective valves. In the acts just

described, on the contrary, the mouth is closed by the pendulous vail of the palate, now performing the office of a valve in a different situation, whilst the windpipe and nostrils communicate and remain open. We thus see the pendulous vail of the palate endued with a double office, the first being accurately to close the nostrils; the second, to close what has been termed the isthmus faucium. In the articulation of certain letters, again, we shall have occasion to observe, that the posterior orifices of the nostrils are closed, whilst the orifice into the windpipe is, of course, left freely open;—so wonderfully and variously are the functions of these delicate organs capable of being combined.

I now proceed to the more immediate object of this essay, namely, to explain the physiology or mechanism of articulation. I shall, first, notice the effect of speech on the respiration, or rather, on the act of *expiration*, which, it will be observed, is in some cases completely interrupted and arrested; in others, performed through the nostrils; in others, again, through the mouth alone. I shall, in the second place, describe the offices of the different parts comprising the organs of speech, in the articulation of the principal *consonants*; and in the third place, I shall trace the action and function of these parts in the enunciation of certain *vowels*.

We shall cease to be surprised at the fatigue expressed by persons whose office it is to speak much in public, when we have duly and fully examined the nature of the function of articulation. It may be ascertained, by the merest experiment, that in the pronunciation of the short word BAT, we adopt a mechanism, by which, not only are the different letters formed, but the respiration is twice completely arrested;—and that, in the pronunciation of the equally short word FAN, we first interrupt the flow of the air through the nostrils, whilst it is forced between the teeth and upper lip, and then intercept the course of the air through the mouth, and allow it to pass only through the nostrils.

Speech might be considered, indeed, first, as an exertion and trial of the muscular power; and secondly, as exerting a baneful influence, in certain cases, on the lungs themselves. The

trial of the muscular power, during speech, is known from the experience of public speakers, but is only fully appreciated by the physician, in his attendance on those cases of disease which particularly impair the muscular strength, as continued fevers; the degree of energy of the speech may, indeed, be considered an accurate measure of the degree of muscular power or debility, and the observant physician may learn from this alone, that his patient is getting worse, remains stationary, or is becoming convalescent. It is not less observable, that speaking has a baneful influence on the patient who labours under disease of the lungs; and it is said of the celebrated Talma, that he never performs *Les fureurs d'Oreste* without being taken with spitting of blood.

It is on their influence on the respiration, that I have formed my division and arrangement of the consonants; their sub-division may be founded on their respective modes, or mechanism of their enunciation. I shall, therefore, divide them—

1. Into those, in the articulation of which both the mouth and the nostrils are closed, and the respiration, of course, completely arrested:

2. Into those, in the enunciation of which the nostrils are closed, but the mouth left more or less open, for the exit of the air, which is compressed, but not interrupted, in its expiration:

3. Into those, not requiring even the nostrils to be closed, and in the enunciation of which the air is still less compressed in its course from the lungs: and,

4. Into those, in the articulation of which the expired air is not interrupted, and scarcely impeded at all.

Of the *first* class, are

B	T	C
P	D	K

In tracing these letters into their sub-divisions, we may observe, that the first pair are labials, being formed by the lips compressed together; the second pair are linguo-dentals, formed by pressing the point of the tongue against the posterior and upper part of the upper teeth; and the third pair are linguo-palatial, being effected by pressing the middle part of the tongue against the

palate. In all, the posterior apertures of the nostrils are effectually closed by the pendulous vail of the palate being drawn upwards, and accurately applied to their posterior apertures. And of course, those persons whose palate is perforated, or in whom the pendulous vail of the palate is imperfect, as sometimes arises from disease, are more or less incapacitated from pronouncing these letters, the expired air being no longer intercepted, as it ought to be, in its course.

Of the *second* class, are

$$\frac{\text{F},}{\text{V}}\ \text{the}\ \begin{matrix}\text{hard}\\ \text{and}\\ \text{soft}\end{matrix}\ \text{TH, and}\ \frac{\text{S}}{\text{Z}}.$$

In the articulation of these letters, the posterior orifices of the nostrils are required to be closed, whilst, in the first pair, the compressed air is continually forced between the teeth and upper lip; in the second, between the teeth and the tongue; and in the third, between the point of the tongue and the anterior part of the palate.

From this view of the subject, it will be readily apprehended, how the substitution of D or T for the TH, by foreigners, is so remarkable; for it is no less than the substitution of a total interruption, for a mere compression of the air in its exit from the chest.

Of the *third* class of letters, are

M, N, L, R.

In the enunciation of these letters, the expired air is only very slightly compressed, the nostrils being left freely open. It is for this very reason, probably, that these letters have been termed *liquids*, as flowing without obstacle. And it is by this circumstance, principally, extraordinary as it may appear, that the letter M differs from the letters B and P, for they are all equally labial; and that the letter N differs from T and D, for they are all equally formed by placing the point of the tongue near the roots of the upper teeth.

Of the *fourth* and last class, are

H, the Greek X, W, and Y.

In the enunciation of these consonants, the air appears to be scarcely compressed or impeded in its exit at all. This fact may, I think, account for the circumstance, that it has even been doubted, whether the two last letters be really consonants or no; and for the remarkable fact, that they cannot, as *consonants*, form the termination of any word.

These letters, preceded, as they are in this arrangement, by the liquids, lead us almost insensibly to the class of letters to be next noticed, namely, the *vowels*.

These are so called, from having been supposed to relate to the *voice* alone*. This, however, is obviously an error. The different parts forming the mouth, or organ of *speech*, are not less necessary to the enunciation of the vowels than to that of the consonants, or their function less appreciable, on carefully making the experiment. Thus, the French U is entirely labial; the letter E is dental; O, palatial; whilst the diphthong AW, and the vowels marked in the French language by the circumflex (∧) are guttural.

The mechanism of the vowels is not, indeed, so obvious as that of the consonants. It is, however, sufficiently so to afford an illustration and explanation of several remarkable facts.

First, it may be observed, that any peculiarity of *articulation* in a language, imparts a peculiar expression or physigonomy to those who speak it. This is particularly observable in the enunciation of the French U, of which we have already spoken, which would not be the case if this letter were merely *vocal*.

Secondly, this fact is so certain, that a child may be taught to form such a letter at once, if his attention be taken from the book and directed to the countenance of the teacher, when it would be quite a task to effect this object in any other manner.

It may be observed, that when any given *vowel* sound exists in a foreign language, and not in our own, it is learnt with far greater difficulty than a new but distinct consonant would probably be; and the same observation would appear to apply to

* Blumenbach.

those consonants which are pronounced with but little compression of the expired air.

I thought it not improbable, at one time, that the pendulous vail of the palate was in part propelled against the posterior orifices of the nostrils, by the force of the expired air, in the act of pronouncing those letters which require the respiration to be arrested for their articulation. But this opinion is altogether disproved by the experiment of articulating these letters during *inspiration*. The course of the air is not less interrupted in this case than in the natural mode of speaking; so that it is plain, that it is by the action of its muscles alone, that the pendulous vail of the palate performs its functions.

The facts stated in the preceding part of this essay have been greatly confirmed by observing the effects of a perforation of the palate, which I have just had the opportunity of doing. The perforation was about equi-distant from the teeth and from the vail of the palate. The following phenomena were observed:— The patient could not swallow, or smoke, or whistle; on attempting to pronounce the letters B, D, S, V, &c., the action of the muscles of articulation was extremely imperfect, and attended by a hissing noise occasioned by the escape of the air through the perforation in the palate. The letters G and K could, however, be pronounced perfectly, the tongue being brought, in the enunciation of these letters, into contact with the palate, at a part posterior to the situation of the perforation.

Having made these observations on the physiology or *mechanism* of speech, I shall conclude this essay by several remarks, which may, probably, lead to some practical utility in the *education* of the faculty of speech, if I may be allowed that expression.

1. *Infancy* is, for two reasons, the period of life at which alone it is possible to teach the pronunciation of a foreign language perfectly; for it is at this age that the functions of the muscular system generally are fully developed, and that those of any particular part of this system are most readily confirmed into an easy habit. This fact is illustrated in the acquisition of execution in music, which is attempted in vain, even during a long

series of years, by any person after a certain age. The same observation applies to the pronunciation of a foreign language. It must be acquired early, or it will never be acquired at all. The French emigrants, who left their native country rather late in life, never acquired the pronunciation of our language, even so as to be readily understood. It is in infancy too, that the faculty of *imitation*, by which the mechanism of articulation is, in fact, principally, if not entirely, caught, exists in its greatest degree of excellency.

2. It has been observed, in regard to *stammerers*, or those who have a defective utterance, that they can *sing*, or even *read*, without hesitation, although they cannot speak. What is the *rationale* of this fact? I think it will be found to depend on the following principle. *Continuous* muscular action is far more easily effected than that which is interrupted. This principle is even general in physiology. It has been remarked, that a drunken man, or a person affected with that disorder termed St. Vitus's Dance, can *run*, though he cannot walk, or stand still*. In the same manner, a stammerer can sing, which is continuous motion, although he cannot speak, which is interrupted.

Continued muscular motion is also attended with less *fatigue* than that which is interrupted; and this is particularly observed in regard to speech. It is on this account, that there is a tendency in those who speak much in public to acquire a sort of continued sing-song mode of delivery, which it requires good taste and constant exertion to correct. It is on this account, too, that those who cry in the streets, actually acquire a sort of tune, or *cry*, as it is termed; the continued action of the muscles of speech being so much more easy than the interrupted. The same is constantly observed in children, on their first attempts to read.

Let a stammerer, then, observe this rule:—Always to speak in a continued or *flowing* manner, avoiding carefully all positive interruption in his speech; and if he cannot effect his purpose in this manner, let him even half sing what he says, until he shall,

* Heberdeni, Com. p. 93.

by long habit and effort, have overcome his impediment; then let him *gradually*, as he may be able, resume the more usual mode of speaking, by interrupted enunciation. I am persuaded, that this is the principal means employed by those gentlemen who have undertaken to correct impediments in the speech, and it is, undoubtedly, the most rational. In addition to this rule, let the stammerer endeavour to speak in as calm and soft a tone as possible; for in this way the muscles of speech will be called least forcibly into action, and that action will be least liable to those violent checks or interruptions, in which stammering appears to consist. It would, of course, be irrelevant to the object of this essay, to allude to those other principles connected with stammering, such as nervousness, of which it would be necessary to treat, in an essay written expressly on this interesting subject.

Art. III. *On some Cases of the Formation of Ammonia, and on the Means of Testing the Presence of Minute Portions of Nitrogen in certain states.* By M. Faraday, F.R.S., Corr. Member Royal Acad. Paris, *&c. &c.*

[Communicated by the Author.]

The importance of the question relative to the simple or compound nature of any of the substances considered as elementary in the present state of chemical science, is such as to make any experimental information respecting it acceptable, however imperfect it may be. An opinion of this kind has induced me to draw up the following account of experiments relative to the formation of ammonia, by the action of substances apparently including no nitrogen. The experiments are not offered as satisfactory, even to myself, of the production of ammonia without nitrogen; indeed, I am inclined to believe the results all depend upon the difficulty of excluding that element perfectly, and the extreme delicacy of the test of its presence afforded by the formation of ammonia: yet as, on the contrary, notwithstanding my utmost exertions, I have failed to convince myself that ammonia could not be formed, except nitro-

gen were present, it has been supposed that the information obtained, though incomplete, might be interesting.

Having occasion, sometime since, to examine an organic substance with reference to any nitrogen it might contain, I was struck with the difference in the results obtained, when heated alone in a tube, or when heated with hydrate of potassa: in the former case no ammonia was produced; in the latter, abundance. Supposing that the potash acted, by inducing the combination of the nitrogen in the substance with hydrogen, more readily than when no potash was present, and would, therefore, be useful as a delicate test of the presence of nitrogen in bodies, I was induced to examine its accuracy by heating it with substances containing no nitrogen, as lignine, sugar, *&c.;* and was surprised to find that ammonia was still a result of the experiment. This led to trials with different vegetable substances, such as the proximate principles, acids, salts, *&c.*, all of which yielded ammonia in greater or smaller quantity; and, ultimately, it was found, that even several metals when treated in the same way gave similar results; a circumstance which appeared considerably to simplify the experiment.

The experiment may be made in its simplest form in the following manner: put a small piece of clean zinc foil into a glass tube closed at one end, and about one-fourth of an inch in diameter; drop a piece of potash into the tube over the zinc; introduce a slip of turmeric paper slightly moistened at the extremity with pure water, retaining it in the tube in such a position that the wetted portion may be about two inches from the potash; then holding the tube in an inclined position, apply the flame of a spirit lamp, so as to melt the potash that it may run down upon the zinc, and heat the two whilst in contact, taking care not to cause such ebullition, as to drive up the potash; in a second or two, the turmeric paper, will be reddened at the moistened extremity, provided that part of the tube has not been heated. On removing the turmeric paper and laying the reddened portion upon the hot part of the tube, the original yellow tint will be restored: from which it may be concluded that ammonia has been formed; a result confirmed by other modes of examination to be hereafter mentioned.

The first source of nitrogen which suggested itself was the atmosphere: the experiment was therefore repeated, very carefully, in hydrogen gas, but the same results were obtained.

The next opinion entertained was, that the potash might have been touched accidentally by animal or other substances, which had adhered to it in sufficient quantity to produce the ammonia: the alkali was therefore heated red hot, as a preparatory step, and afterwards allowed to touch nothing but clean glass or metals; but still the same effects were produced. The zinc used was selected from a compact piece of foil, was well rubbed with tow dipped in alkali, washed in alkaline solution, afterwards boiled repeatedly in distilled water, and dried, not by wiping, but in a hot atmosphere; and yet the same products were obtained.

All these precautions, with regard to impurity from fingering, were found to be essentially requisite, in consequence of the delicacy of the means afforded by heat and turmeric paper for testing the presence of ammonia, or rather, of matter containing its elements. As a proof of this, it may be mentioned, that some sea sand was heated red hot for half an hour in a crucible, and then poured out on to a copper-plate, and left to cool; when cold, a portion of it (about 12 grains) was put into a clean glass tube; another equal portion was put into the palm of the hand, and looked at for a few moments, being moved about by a finger, and then introduced by platina foil into another tube, care being taken to transfer no animal substance but what had adhered to the grains of sand: the first tube when heated yielded no signs of ammonia to turmeric paper, the second a very decided portion.

As a precaution, with regard to adhering dirt, the tubes used in precise experiments were not cleaned with a cloth, or tow, but were made from new tube, the tube being previously heated red hot, and air then drawn through it; and no zinc or potash was used in these experiments, except such as had been previously tried by having portions heated in a tube to ascertain whether when alone they gave ammonia.

It was then thought probable that the alkali might contain a minute quantity of some nitrous compound, or of a cyanide, intro-

duced during its preparation. A carbonate of potash was therefore prepared from pure tartar, rendered caustic by lime calcined immediately preceding its use, the caustic solution separated by decantation from the carbonate of lime, not allowed to touch a filter or any thing else animal or vegetable, and boiled down in clean flasks; but the potash thus obtained, though it yielded no appearance of ammonia when heated alone, always gave it when heated with zinc.

The water used in these experiments was distilled, and in cases where it was thought necessary was distilled a second, and even a third time. The experiments of Sir Humphry Davy* shew how tenaciously small portions of nitrogen are held by water, and that, in certain circumstances, the nitrogen may produce ammonia. I am not satisfied that I have been able to avoid this source of error.

At last, to avoid every possible source of impurity in the potash, a portion of that alkali was prepared from potassium; and as the experiment made with it includes all the precautions taken to exclude nitrogen, I will describe it rather minutely, as illustrative of the way in which the other numerous experiments were made. A piece of new glass tube, about half an inch in diameter, was first wiped clean, and then heated red hot, a current of air passing at the same time through it; about six inches in length was drawn off at the blow-pipe lamp, and sealed at one extremity. Some distilled water was put into a new glass retort, and heated by a lamp; when about one-half had distilled over, the beak of the retort was introduced into the tube before-mentioned, and a small portion of water (about fifty grains) condensed into it. A solid compact piece of potassium was then chosen out, and having been wiped with a linen cloth, was laid on a clean glass plate, the exterior to a considerable depth removed by a sharp lancet, and portions taken from the interior by metallic forceps, and dropped successively into the tube containing the water before-mentioned. Of

* Phil. Trans. 1807. p. 11.

course the water was decomposed, and the tube filled with hydrogen; and when a sufficient quantity of solution of potash had been thus formed, the tube was heated in a lamp, and drawn out to a capillary opening, about two inches from the closed extremity. (fig. 1. plate.) The tube now formed almost a close vessel; and being heated, as the water became vapour, it passed off at the minute aperture, and ultimately a portion of pure fuzed hydrate of potassa remained in the bottom of the tube. The aperture of the tube was now closed, and the whole set aside to cool.

A piece of new glass tube was selected about 0.3 of an inch in diameter; it was heated to dull redness, and air passed through it: about ten inches of it was then cut off, and being softened near to one end by heat, it was drawn out at that part until of small diameter: (*a*, fig. 2. plate.) that part was then fixed into a cap, by which it could afterwards be attached to a receiver containing hydrogen. The tube containing the potassium potash being now broken in an agate mortar, a piece or two of the potash was introduced by metallic forceps into the tube at the open end, so as to pass on to the contracted part; a roll of zinc foil, about one grain in weight, cleaned with all the precautions already described, was afterwards introduced, and then more of the potash. The tube was then bent near the middle to a right angle; a slip of turmeric paper introduced, so as just to pass the bend, and thus prepared, it was ready to be filled with hydrogen.

The precautions taken with regard to the purity of the hydrogen, were as follows: a quantity of water had been put into a close copper boiler, and boiled for some hours, after which it had been left all night in the boiler to cool. A pneumatic trough was filled with this water just before it was required for use. The hydrogen was prepared from clean zinc, which being put into a gas bottle, the latter was filled entirely with the boiled water, and then sulphuric acid being poured in through the water, the gas was collected, the excess of liquid being allowed to boil over. The hydrogen was received in the usual manner into jars filled with the water of the trough, the transferring jar, when filled, being entirely im-

mersed in the water, so as to exclude the air from every part, even of the stop-cock. The first jar of gas was thrown away, and only the latter portions used.

The gas being ready, the experimental tube was attached to the transferring jar by a connecting piece, so that the part of it containing the zinc and potash was horizontal, whilst the other portion descended directly downwards. A cup of clean mercury, the metal being about an inch in depth, was then held under the open end of the tube, and by lowering the jar containing the hydrogen in the water of the pneumatic trough, so as to give sufficient pressure, and opening the stop-cock, the hydrogen in the jar was made to pass through the tube, and sweep all the common air before it. When from 100 to 150 cubic inches, or from 200 to 300 times the contents of the tube, had passed through, the cup of mercury was raised as high as it could be, so as to prevent the passage of any more gas, the pressure from the jar in the water-trough was partly removed, and the stop-cock closed; then, by lowering the cup of mercury a little, the surface of the metal in it was made lower than that within the tube, and in this state of things the flame of a spirit lamp applied to the contracted part of the tube, (*a*, fig. 2.) sealed it hermetically, without the introduction of any air, and separated the apparatus from the jar on the water-trough.

In this way every precaution was taken that I could devise for the exclusion of nitrogen; yet, when a lamp was applied to the potash and zinc, the alkali no sooner melted down and mingled with the metal, than ammonia was developed; which rendered the turmeric paper brown, the original yellow re-appearing by the application of heat to the part.

Still anxious to obtain a potash which should be unexceptionably free from any source of nitrogen, I heated a portion of potash with zinc, endeavouring to exhaust any thing it might contain which could give rise to the formation of ammonia: it was then dissolved in pure water, allowed to settle, the clear portion poured off and evaporated in a flask by boiling; but the potash thus prepared gave ammonia, when heated with zinc, in hydrogen gas.

With regard to the evidence of the nature of the substance produced, it was concluded to be ammonia in the experiments made in hydrogen, from its changing the colour of turmeric paper to reddish brown; from the disappearance of the reddish brown tint and reproduction of yellow colour by heat; from its solubility in water, as evinced by the greater depth of colour on moist turmeric paper than on dry; from its odour; and from its yielding white fumes with the vapour of muriatic acid. When formed in open tubes, its nature was still further tested by its neutralizing acids and restoring the blue colour of reddened litmus paper; by its rendering a minute drop of sulphate of copper on a slip of white paper deep blue; and also, at the suggestion of Dr. Paris, by introducing into it a slip of paper moistened in a mixed solution of nitrate of silver and arsenious acid, the yellow tint of arsenite of silver being immediately produced.

These experiments upon the production of ammonia from substances apparently containing no nitrogen, will call to mind that made by Mr. Woodhouse, of Philadelphia, on the action of water on a calcined mixture of charcoal and potash, during which much ammonia was produced*; and also to the strict investigation of that experiment made by the President of the Royal Society during his inquiries into the nature of elementary bodies†. Sir Humphry Davy found that when one part of potash and four of charcoal were ignited in close vessels cooled out of contact of the atmosphere, pure water admitted to the mixture, and the whole distilled, small quantities of ammonia were produced. That when the operation was repeated upon the same mixture ignited a second time, the proportion diminished; in a third operation it was sensible; in a fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when the mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Sir Humphry Davy refrains from drawing conclusions from

* Nicholson's *Journal*, xxi. 290. † *Phil. Trans.* 1809, p. 100, 1810, p. 43.

these processes, observing with regard to the composition of nitrogen in these experiments, that till the weight of the substances concerned and produced in these operations are compared, no correct decision on the question can be made: I am anxious to be understood as imitating the caution of one whose judgment stands so high in chemical science; and, therefore, draw no positive conclusion from the experiment I have described, or from the results I have yet to mention. As, however, I think they may lead to elucidations of the question, I shall venture to give them, not with the minute detail of the preceding experiment, but in a more general manner.

Potash is not the only substance which produces this effect with the metals and vegetable substances. Soda produces it; so, also, does lime, and baryta, the latter not being so effective as the former, or producing the phenomena so generally. The common metallic oxides, as those of manganese, copper, tin, lead, *&c.*, do not act in this manner.

Water or its elements appear to be necessary to the experiment. Potash or soda in the state of hydrates generally contain the water necessary. Potash dried as much as could be by heat, produced little or no ammonia with zinc; but re-dissolved in pure water and evaporated, more water being left in it than before, it was found to produce it as usual. Pure caustic lime, with very dry linen, produced scarcely a trace of ammonia, whilst the same portion of linen with hydrate of lime yielded it readily.

The metals when with the potash appear to act by, or according to, their power of absorbing oxygen. Potassium, iron, zinc, tin, lead, and arsenic evolve much ammonia, whilst spongy platina, silver, gold, *&c.*, produce no effect of the kind. A small portion of fine clean iron wire dropped into potash melted at the bottom of a tube, caused the evolution of some ammonia, but it soon ceased, and the wire blackened upon its surface; the introduction of a second portion of clean wire caused a second evolution of ammonia. Clean copper wire, in fused potash, caused a very slight evolution of ammonia, and became tarnished.

The following, among other vegetable substances supposed to

contain no nitrogen, have been tried with potash in tubes open to the air; lignine, prepared by boiling linen in weak solution of potash, then in water, afterwards in weak acid, and finally in water again; oxalate of potassa, oxalate of lime, tartrate of lead, acetate of lime, asphaltum, gave very striking quantities to tumeric and litmus paper: acetate of potash, acetate of lead, tartrate of potash, benzoate of potash, oxalate of lead, sugar, wax, olive oil, naphthaline, produced ammonia, but in smaller quantity: resin appeared to yield none, nor when potash was heated in the vapour of alcohol or ether, or in olefiant gas, could any ammonia be detected.

It may be remarked, that much appeared to depend upon the quantity of potash used; sugar, for instance, which with a little potash would with difficulty yield traces of ammonia, does so very readily when the quantity of potash is doubled or trebled; and linen, which with potash gives ammonia very readily, yields it the more readily, and in greater quantity, as the proportion of potash is increased.

The experiments with the substances which contain carbon, assimilate, in consequence of the presence of that body, with the one by Mr. Woodhouse. Whether the substances act exactly as charcoal does, probably, cannot be decided until the correct nature of the action is ascertained; but there are apparently some very evident differences. The ammonia, in the charcoal experiment, does not exist until after the ignition, nor before the addition of water; but in several experiments of the nature of those described in this paper, the ammonia is evolved before the substances acting or acted upon, are charred. Thus, if linen fibre, cut small, be mixed in the tube with hydrate of lime, and heated, ammonia is evolved before the heat has risen so high as to render the linen more than slightly brown; and oxalate of potash, in a tube with potash, when heated, gives much ammonia before any blackening is produced.

Mr. Woodhouse's experiment may be very readily repeated, though not in an exact way, by heating a little tartrate of lead with potash, in a tube in the flame of a spirit lamp, driving off the

water and first products, and raising the residue to dull redness. If a drop of water be allowed to flow down on to the residue when cold, and it be then heated, ammonia will be found to rise with the water.

I was induced in the course of these experiments to try again and again, whether the potash or lime would not yield ammonia when heated alone; but when well prepared, and the tubes experimented in perfectly clean, they gave no indications of it. By exposure to air for three days in a room, hydrate of lime appeared to have acquired the power of evolving a little ammonia when heated, and caustic lime so exposed gave still stronger traces of it. Potash also exhibited an effect of this kind, and potash which had been heated with zinc, and contained oxide of zinc, mostd ecidedly. Some potash and zinc were heated together; a part was immediately put into a clean close bottle; another part was dissolved in pure water, decanted, the solution evaporated in a covered Wedgewood's basin, and then also set aside in a close vessel for 24 hours: at the end of that time the first portion, heated in a tube, gave no decided trace of ammonia, but the latter yielded very distinct evidence of its presence, having apparently absorbed the substance which was its source from the atmosphere during the operations it had been submitted to. White Cornish clay being heated red hot, and then exposed to the air for a week, gave plenty of ammonia when heated in a tube. When the substances were preserved in well-stoppered phials, these effects were not produced.

Such are the general and some of the particular facts which I have observed relative to this anomalous production of ammonia. I have refrained from all reasoning upon the probability of the compound nature of nitrogen; or upon what might be imagined to be its elements, not seeing sufficient to justify more than private opinion on that matter. I have endeavoured to make the principal experiments as unexceptionable as possible, by excluding every source of nitrogen, but I must confess I have not convinced myself I have succeeded. The results seem to me of such a nature as to deserve attention, and if it should hereafter be proved

that nitrogen had entered in some unperceived way into the experiments, they will still shew the extreme delicacy of heat, or heat and potash, as a test of its presence by the formation of ammonia.

With respect to the delicacy of the test, it may be observed that it offers many facilities to the detection of nitrogen when in certain states of combination, which chemists probably were not before aware of. A portion of asbestos, which had been heated red hot, was introduced into a tube by metallic forceps and heated, it gave no ammonia; another similar portion compressed together, and introduced by the fingers, gave ammonia when heated. A very minute particle of nitre was dropped into hydrate of potassa, and heated to dull redness, it gave no ammonia; a small piece of zinc foil, dropped in and the heat applied, caused an abundant evolution of that substance.

The circumstance also of absorption by lime and other bodies, of something from inhabited atmospheres, which yields ammonia when thus tested, is very interesting; and Dr. Paris has suggested to me that this power may probably be applicable to the examination of the atmosphere of infected and inhabited places, and may perhaps furnish the means of investigating such atmospheres upon correct principles.

February, 17, 1825.

Art. IV.—*Description of the Coal recently discovered on the Estates of the Count de Regla, in the Intendancy and Kingdom of Mexico. By* Th. Stewart Trail, M.D., F.R.S.E., *&c.*

[Communicated in a Letter to Mr. Swainson.]

The mineral treasures now laid open to the skill and enterprise of British adventurers in South America, are daily exciting an increased interest throughout the kingdom. And as connected with the powerful machinery that will be employed in these un-

dertakings, the subject of FUEL becomes one of the greatest importance. The woods and forests, which once clothed the sides of the Cordilleras in the vicinity of the principal mines, have been, for many years, gradually diminishing, and in many places have totally disappeared; while the Mexican proprietors, with singular negligence, have forgotten to form new plantations to supply that enormous quantity of fuel necessary for the mines.

The existence of Coal on the mining provinces of Mexico has hitherto been very doubtful. Humboldt, indeed, mentions it has been found in New Mexico; and that the formations of basalt and amygdaloid on the estates of the Count de Regla might lead to the belief that this substance also would probably be discovered; a supposition likewise entertained by Mr. John Taylor, whose practical and scientific knowledge of mining is well-known. These opinions are now completely verified; as among the mineral productions brought by Mr. Bullock from Mexico, are specimens of a coal analogous to jet, which he procured while residing in the vicinity of Real del Monte. A small piece of this substance, weighing sixteen grains, has been analyzed by Dr. Trail, and the result of his experiments, contained in a letter to Mr. Swainson, is expressed in the following words:—

"This specimen is more analogous to *jet* than to our Wigan *Cannel* coal. Its colour is deep brownish black; its lustre resinous; its cross fracture conchoidal; its longitudinal fracture has a slightly fibrous appearance, as if it had originally been wood. Its hardness is about that of Cannel coal, as is its frangibility; but its lustre is higher. The mean of three careful experiments gave its specific gravity=1.2248. It becomes considerably electric by friction; in this character it is analogous to *jet*, and differs from *Cannel coal*, which scarcely shews any symptoms of electricity by friction. Though I have observed that some pieces of the latter slightly moved an insulated cat's hair, which is a very delicate electroscope. Kirwan considers the difference between *jet* and Cannel in their electric energies, as a diagnostic mark.

"It burns with a lively flame, and gives out much liquid

bituminous matter, or *coal tar*, so as to cake or become semi-liquid in the fire. It does not decrepitate when heated, like Cannel· When heated before the blowpipe, in a glass tube, its volatile parts are separated; and it leaves behind about 50 per cent. of a coke which is capable of exciting a pretty strong heat. The volatile portion affords a *very pure* coal gas. Six grains of it, burnt in a platina crucible, left behind 0.2 grains of greyish white ash, which is equivalent to 2¼ per cent. of incombustible matter in it.

" The smallness of the specimen rendered it impossible to ascertain the relative quantities of carbon, hydrogen, and nitrogen, which similar substances contain, but this sort of analysis is rather an object of curiosity than utility."

V.—*On the Origin, Materials, Composition, and Analogies of Rocks*, by John Mac Culloch, M.D., F.R.S.E.

[Communicated by the author.]

If it is the first error of the observer to see, like the miner, but a very limited number of rocks in the system of nature, it is not long before he falls into one, the very reverse; creating for himself permanent distinctions from every incidental variety that comes under his notice. Time, however, speedily corrects this error, and teaches him, that however the aspects of rocks may be multiplied, Nature has limited these productions by a very confined set of general and constant characters.

Of the Constituents of Rocks.

A small number only of the earths which chemistry has discovered, forms the materials of all the rocks; united, in some cases, with alkalies, and with certain metallic oxides. In some, a single earth is [illegible]; in others, two or more exist; and these are either mechanically mixed, or united by the laws of chemical affinity. Thus are formed those rocks which are considered simple; simplicity, as applied to rocks, meaning simplicity of

aspect. Limestone presents an example of a rock in every respect simple; while basalts and clay slates, although simple as rocks, are chemical compounds and mechanical mixtures.

Besides these distinctions, the earths are sometimes formed into separate minute bodies, or minerals, which are again united, so as to constitute rocks; and these may be in themselves either simple or compound minerals. Sandstone offers an example of a simple rock of this kind; simple in its chemical nature, but an aggregate as to its general character. Hornblende rock is an example of an analogous aggregate, but one in which the integrant minerals are chemical compounds. But there are differences here, even in the mode of aggregation: which, in some cases, results from the chemical interference of a simultaneous crystallization; in others, from the mere mechanical aggregation of the parts; and lastly, from the union of those two processes. Granular limestone is an example of the first, and instances of the last are to be found in different varieties of sandstone.

In compound rocks, different kinds of minerals are visibly united into a common mass; which thus presents a sort of uniformity throughout the whole, however the separate parts may differ. Such compounds may consist of two or more minerals; and, within certain limits, they seem to be ruled by laws as general as the simpler rocks. These compounded rocks vary, like the former, in being purely crystalline or otherwise; and as granite presents a familiar example of the first, so quartz rock, and some of the compound argillaceous schists, afford instances of the other two.

There is still another description of compound rocks, to which the term conglomerate has been applied. In these, not only different minerals are united in a mechanical, a mixed, or a chemical manner, but fragments of former rocks, either simple or compound, also enter into their structure. Such fragments vary in size, from the most minute visible particles, to others of many pounds, or even hundreds of pounds weight; and these rocks offer, in consequence, numerous varieties, which are fully treated of in the author's Geological Classification of Rocks;

the repository for the facts of this description which do not appertain to the present paper.

The earths, which enter into the composition of those minerals that form the ordinary or essential ingredients of rocks, are silica, alumina, lime, and magnesia. If the other earths are occasionally found, it is rather in those minerals which cannot be considered essential to the constitution of rocks, but which are frequently imbedded in them. To these earths must be added, iron in different states of oxidation; and, from some observations which I have made on quartz rock, limestones, and traps, in that of a carbonate also. Potash and soda are, lastly, essential ingredients in some rocks, and it remains to be proved whether lithia may not sometimes be present where one or other of these has been suspected. As the earths, as well as the alkalies, are now known to be oxides, and as it is also known that silica, at least, acts the part of an acid in some mineral combinations, it is probable that we have much yet to learn, respecting the origin and formation of many rocks; but whatever splendid probabilities may open on us from this new source of knowledge, we are scarcely yet able to build any rational conjectures on it.

The simple minerals formed of these substances, and which constitute the essential ingredients of all rocks, are quartz, felspar, mica, hornblende, hypersthene, diallage, augit, serpentine, compact felspar, actinolite, chlorite, talc, and schorl. Some of these are, however, far more abundant than others; nor is it easy to define the limit between them and those which may be considered accidental; which are occasionally imbedded in rocks, as their natural repositories. But it is unnecessary to dilate on a subject which is sufficiently detailed in the work above-mentioned. It is sufficient to quote, as examples, garnet, which is sometimes abundant in micaceous schist, or sparingly dispersed, or altogether absent, without affecting its essential characters; and spodumene or corundum, which may thus exist in granite.

If we consider the great number of minerals, thus generally distinguished into essential and unessential, which nature has formed, or if even we limit our views to those which may be

considered as most essential, it is interesting to observe how few are the rocks which are produced from them. If the varieties are most numerous in the primary or older series, they are still few, and, within certain limits of variation, very constant. In the later rocks, the varieties are still more limited; and, when we reflect on the circumstances under which they have been produced, they are confined to a much less number than could have been expected. As most of the minerals of ordinary occurrence are formed, for example, of the earths which exist in granite and gneiss, why might we not expect to find garnet, corundum, or andalusite, in every one; instead of being, as they are, limited, to a few occasional specimens. We are equally at a loss to account for those distinctions between gneiss, micaceous schist, quartz rock, or other substances, that occur in the same ancient series; distinctions which, on the great scale, are really steady and definite, notwithstanding the occasional interferences of character that occur in particular cases. That these have been regulated by certain chemical laws, is unquestionable, however incomprehensible the nature of these may be. It must also be from this cause, that such rocks are found to preserve the same characters, wherever they occur; a circumstance otherwise calculated to excite our surprise. In every other department of nature, her productions vary according to the climate and situation, but granite is the same in Egypt and in Greenland. It is with the laws of organization alone that climate interferes.

As the secondary, or later, strata have been chiefly formed from the waste of the ancient rocks, it is less surprising that they should preserve a general constancy of character throughout the globe, however individuals may vary in different places. Even these variations are still remarkable; as well from their steadiness, as from the extent through which that uniformity can sometimes be traced. The difference between compact limestone and chalk, is no less remarkable than the similarity which, in distant places, occurs between strata that we can scarcely conceive to have fo[illegible] parts of one deposit. It is worthy of remark, however, that, in the secondary strata, the most conspicuous

variations occur in the limestones; and these, it is obvious, have been subject, in many instances, to chemical laws, as well as to the influence of organized bodies, from which the others have been comparatively exempted. That the secondary strata should contain sandstone and schists, is easily accounted for, by recollecting that these must be the result of the destruction of the older rocks ; the more durable mineral remaining distinct, while the compound ones have been reduced to powder. But the question of the formation and origin of all these rocks will be examined more particularly hereafter.

On the Consolidation of Rocks.

As almost all the rocks with which we are acquainted have been formed out of our sight, the mode in which the earths, or simple minerals, became consolidated into these forms, is to us a matter of inference from analogy, not of observation. If discussion could have determined this question, it would have been solved long since ; as most of the schemes which have been called Theories of the Earth have been chiefly engaged in this pursuit, and as neither argument nor assumption has been spared in attempting to establish the exclusive views of many of these theorists. To record the terms under which the different partisans have thought fit to array themselves, would be to foster and perpetuate an opposition, often arising, more, perhaps, from the colours of the different banners, than from the merits of the cause. Blue and green factions have often exerted their influence beyond the limits of eastern or western empires, and in far other pursuits than politics.

Fortunately, all rocks have not been formed in the depths of the earth, and fortunately, also, it is in the power of art to produce some of these substances from indiscriminate mixtures of their elements. It is our business to try how far we can extend analogies from the visible to the invisible, from the present to the past. If this process will not carry us far, it is at least the only rational mode of investigation in our power.

Volcanoes are among the most active and impressive sources

of those rocks which are now daily forming on the surface of the globe. By the agency of their fires, the earths are ejected in a state which, as far as we know, is merely that of mixture, and united in the fluidity of fusion. By repose, during a process of slow cooling, various combinations take place in these fluid masses; and, according to circumstances which we are but imperfectly able to appreciate, there are formed numerous rocks, either apparently simple, or compounded of the different minerals that have been formed by the contending affinities of the materials. These processes are imitable by art; which, having first reduced the natural compounds furnished in basalt or other rocks, to a fluid and uniform glass, in the laboratory fires, disposes them so as to cool during long repose, in a gradual manner. Thus, by the slow cooling of the most compounded materials of the glass-house furnace, various imitations of rocks are formed; and thus, more precisely, the greenstones of the trap family are destroyed, and again regenerated.

In examining now those rocks which have been formed out of our sight, we find one family which produces many counterparts to the volcanic rocks, namely, the family of trap. So absolute, indeed, is the identity between many members in each set, that no eye nor any analysis can distinguish them. To attempt to prove this by an enumeration of specimens in each, would be only to give a list of names that would carry no conviction. But no more convincing proof is wanted than this; that, to this moment, geologists continue to dispute about what belongs to the trap family, and what is of volcanic origin; not only in countries remote from volcanoes, or no longer containing the marks of former activity; not only in the Vivarais and the Euganean hills, but at the very seats of living volcanoes. If, therefore, out of a common mass of rock, or among many different ones evidently formed under the same circumstances, there are parts which bear all the marks of an origin similar to that of volcanic rocks, it is evident that the whole must be referred to the same source, with certain exceptions arising from collateral circumstances that it is not within the limits of this paper to notice.

Thus analogy, resemblance, and experiment, confirm that opinion respecting the trap rocks, which would be inferred from the peculiarities of their chemical constitution, and thus also they confirm the conclusions that may be drawn from their peculiar disposition, and from the nature of their connexion with the various conterminous rocks among which they are found.

It is but a step from the trap rocks to granite; and if the identity of specimens is not always so perfect, or the resemblance so general and extensive between these and the volcanic rocks, the analogical reasoning is quite as unexceptionable. I showed, in a former paper in this Journal, that many rocks, forming integrant portions of a granite mass, are undistinguishable from many of the trap rocks, and that among these there are many that resemble the productions of volcanoes. Here, then, is an identity, even between granites and volcanic rocks; and here, also, what is true respecting the origin of one part of the mass, must be true respecting the whole. If that inference appears to be drawn closer than the circumstances seem to warrant, we may carry it through the intermediate stage of trap; and having thus proved the identity of this rock with the volcanic products on the one hand, and with the granite on the other, apply a common mathematical axiom to the conclusion.

If it be said that volcanoes do not produce granite, it must still be recollected that they produce compounds of an analogous nature in every respect. It was also shewn, in the paper referred to, that the trap rocks often assumed the character of perfect granite; so that, by this intermediate step, the several products which are most distant are again associated. Even admitting that the volcanic rocks stood exclusively at one extremity of a scale of chemical compounds, and the granites at the other, the trap rocks containing examples of both, form the common link by which they are united. This view of the chemical origin of granite is confirmed by the same set of appearances which confirm it in the case of the trap family, and which have been sufficiently described by various writers.

It is not difficult to assign probable reasons for the differ-

ences in the chemical appearances of the rocks in these three distinct productions. They have, however, been sufficiently pointed out, on other occasions; and it has been shewn, that they probably consisted, in a great measure, in differences of the time through which the fused materials had cooled, circumstances confirmed by a great number of collateral appearances, although, in many cases, there can be no doubt that great differences have resulted from the different proportions of the several earths in the fused compounds.

Thus, from chemical analogies, there is assigned to all the unstratified rocks, that origin which was already deduced from various other considerations; and thus there is proved to exist a division of rocks formed exclusively by the agency of heat. It will now be convenient to begin the remainder of this examination at the other extreme.

Where water holding carbonate of lime in solution is gradually evaporated, there are formed calcareous concretions, which often attain a great size through age, and which, under peculiar circumstances of crystallization, are sometimes not very different in aspect from certain limestone rocks. Under different circumstances, similar waters deposit their contents, so as to form rocks of great depth and extent, producing real calcareous strata. The Travertino of Italy appears to be one of the most perfect examples of this nature. These simple and recent calcareous rocks become compounds, in cases where the calcareous solution has entangled fragments of shells, as it does in the West Indian islands at this day, or where it has united fragments of discordant natures, as it does on the shores of Messina, and on many of our sea coasts. Thus calcareous rocks, both simple and compound, are formed by water. Lastly, rocks of this nature are now daily produced, in many parts of the great ocean, by the efforts of marine animals; the deserted coralline structure being cemented partly by the actions of the animals themselves, and partly by that of the sea on the calcareous earth. In the same manner, ancient submarine piers, as at Carthage, become cemented through lapse of time, by the intervention of shell-fish

and the solution of their calcareous matter, into masses of solid rock. In this way, calcareous rocks are formed, partly by chemical agency, and partly by that of submarine animals.

Where iron becomes converted from the metallic or oxidulous state to that of rust, it becomes the cement of all the smaller materials within its reach; and thus sandstone is often formed on sea-shores, in sand and gravel beds, and, very probably, to a considerable extent in the noted ferruginous sand stratum of England.

Thus two modes of producing rocks, by the agency of water, are demonstrated. It remains to inquire, what probability there is, that the same agency can convert silica to that end, as we cannot produce any instances so perfect, of its absolute action in that way.

The solubility of silica in water cannot be a matter of dispute, however difficult it may be to effect its solution in our laboratories. In my work on the Western Islands, I have produced nearly all the instances of this nature that are required for the present purpose; but I may here add to these, its actual solution in the hot waters of Iceland and Italy, and the consequent production of siliceous tufas and stalactites. To convert this property to the present purpose, it is not requisite that the solution be very extensive, or very rapid. If we conceive this agent acting for a long series of years in a mass of loose sand or of clay, it is not difficult to see that the final result must be, in the first instance, the formation of a sandstone, and, in the other, probably, that of a schist.

That this is the fact, in nature, is almost demonstrable, from the frequent partial occurrence of sandstones in beds of loose sand, and from the mixed chemical and mechanical texture of almost all the solid sandstones. This effect, it is true, has sometimes been attributed to the action of heat; but to adduce as an agent that which cannot be shewn capable of producing a given effect, while we are in possession of one that has the desired power, is to abandon sound reasoning for the sake of maintaining a species of fictitious analogy, which, after all, is not ne-

cessary for the support of that theory by which it was so anxiously defended.

Thus there have been produced two distinct sets of causes for the formation of rocks; the first chiefly applicable to the unstratified substances, and the last to the formation or consolidation of strata.

Mr. Playfair has objected to the possibility of aqueous consolidation on these grounds; that a liquid solvent could not exclude itself from the pores of the rock after depositing the consolidating matter, and that it should, therefore, remain within the stone, or else leave the body pervious to water; "neither of which is" said to be "the fact." On the contrary, both of these propositions are true. The presence of water in stones is so universal, that I have never yet found any rock in which it did not exist, when that could be procured quickly from a sufficient depth. It is contained even in granite, and in the trap rocks; and the great change of colour and hardness which many of the latter undergo after being formed into specimens, is owing to its evaporation. Thus specimens of augit rock, which have the waxy soft look and green colour of serpentine, when fresh broken, become black in a few days. It is also known, that small granite veins are sometimes found perfectly soft in the quarry; and these harden in a few days, apparently by the evaporation of their water, and the consequent precipitation of silica, or else by the nearer approximation of their parts. In Sky, I have found masses of granular quartz or sandstone, which could be moulded by the hand when first found, but which, in the same manner, became solid in a few days. In all these cases, the loss of weight proves the presence of water, as it does the porosity of the stones. Even the common quartz of veins contains water, under the same circumstances; losing both weight and transparency on drying. The porosity of stones, as well as the presence of water, are thus both proved by the same facts, that the former property ought never to have admitted a doubt; since the compactness of flint and agate are apparently far greater than that of any rock, compound or simple, and since these not only give pas-

sage to oil, but even permit sulphuric acid to follow, and to precipitate the charcoal within the pores of the stone.

That the water in stones is actually saturated with earths, and probably with silica or lime, appears to be also proved, by certain appearances which take place on breaking and drying some of these. In marbles raised very wet from the quarry, a whitish dusty surface soon follows, from the deposition of the carbonate of lime; and it is probable, that a similar cause will account for that gray tarnish which is produced in pitchstones, within a very few hours after the specimens are broken from the rock; during which process of drying, they also become far more compact, or less tender. Thus the objection in question falls to the ground; were it even necessary that the process of consolidation should be reserved for that time at which the whole stratum was completed.

Of the different Rocks, and the Modes of their Consolidation.

Now although a large portion of the strata of the globe may have been formed by this last process of aqueous solution, and a considerable portion, at least, of the secondary ones probably owe their origin or consolidation to this cause, there are many strata, particularly in the primary or older series, to which it is impossible to apply it, so as to explain all the appearances which they present. It will here be convenient to point out in succession, those which may have been consolidated merely from water, ending with those which will not admit of that explanation; and it will remain to inquire, whether the phenomena cannot be explained by the successive agency of both the causes which have been examined. It will also be seen, that in one instance, at least, among these ancient strata, either cause separately might have produced the effects visible.

There is nothing in the character of quartz rock, as far as I have examined it, to prevent it from having been consolidated to its present condition from the long continued application of an aqueous solution of silica. But that it was deposited from water originally in the state of sand and gravel, is rendered evident

from the rounded and foreign fragments of discordant rocks which it often contains. At the same time, there is no reason to deny that it may have been exposed to the action of heat; as it is still capable of undergoing that without suffering any change. That it was consolidated by heat we cannot prove; and are scarcely in a condition to deny, that it may not have been partly indebted for its constitution to that cause.

If shale could be indurated from water alone, there would be no reason to deny that the same cause may have operated in the primary argillaceous schists; while, that they have been deposited from water, is proved by the fragments and the shells which they so often contain. Here again, however, we are in the same condition as with regard to quartz rock; unable to prove that it may not have experienced in some degree the action of heat; as we know, from observations on the siliceous schists, that shells are not necessarily obliterated in these circumstances. But that action, if it existed, cannot have been very great; as we are certain, both from experiments and observation, that it is either fused or indurated by this cause. The very existence of siliceous schist in the vicinity of trap and granite, produced by the action of these rocks on shale and slate, not only prove this fact, but shew the very limits where the action of heat ceases.

Thus, two important members of the primary strata are, probably, indurated from water alone. With respect to limestone, it is now known, both by direct experiment and by observation on the effect produced by trap veins on chalk, that it may be crystallized from fusion, provided the escape of the carbonic acid is restrained. It has been shewn, that it is equally consolidated from water; and, on examining this limestone in its various associations, its origin must probably, in some instances, be referred to one of these causes—in others, to the other. It is likely, for example, that all the limestones associated with clay slate are derived from watery deposition and crystallization; and it is probable that those associated with gneiss have received their present condition from heat. This opinion is justified by many circum-

stances; such as by their giving passage to granite veins, by the change of chemical texture and composition which they present in these cases, and by the crystallization within them of minerals similar to those found in gneiss, such as garnet, hornblende, augit, and others, which could not have been deposited from water so as to have entered into the confused crystalline arrangement of the rock in the manner which they do. That limestone is actually thus consolidated after fusion, even in large masses, is also proved, as far as anything relating to the influence of trap is proved, by the conversion of conchiferous secondary strata, in those situations, into crystalline limestone; a fact occurring very extensively and demonstrably in Sky, and recorded in my work on the Western Islands.

With respect to serpentine, the whole question is as yet involved in darkness. It is not known that it can be formed from water, and I have proved from observation, that, as it passes into trap, forming part of a greenstone vein in Perthshire, it can be formed by fusion.

All the scaly schists, of which micaceous schist may here represent the whole, present characters which are scarcely explicable without admitting the action of these two agents. The stratified disposition, and the laminar form, both give indications of a deposition from water; and, if any doubt of that could remain, it is removed by finding that, in many places, it contains fragments of discordant rocks,—of granite, for example, limestone, and quartz rock. It has further been held, that the parallel position of the mica is, in itself, a sufficient proof of deposition, because it is the necessary position, and because the same circumstance exists in the micaceous sandstones, so analogous to it, which are actually deposited from water. But this, if probable, is an equivocal circumstance: as I have shown, that in hypersthene rock, a member of the trap family, and even in some rare trap veins that contain mica in the Western Islands, the flat crystals of hypersthene in one case, and the mica in the other, preserve that parallelism which must here be attributed to the polarity of crystallization operating extensively; an action which I have also elsewhere

shown to have been sometimes exerted throughout the felspar of granite veins.

But admitting that micaceous schist was deposited, like the secondary micaceous sandstones, from water, and consolidated by the same means, it presents characters which cannot be explained by this process. If its flexibility has not been the consequence of heat, which I have elsewhere attempted to prove that it has, the peculiarities of its crystalline texture and occasional contents cannot be explained, without admitting that it has been exposed to a heat sufficiently intense and sufficiently durable, to permit these minerals to be formed in the same manner as they are in granite and in the volcanic rocks. The condition and existence of garnet, hornblende, tourmalin, staurotide, and other minerals, are inexplicable by any mode of watery deposition, and still less by any subsequent crystallization from water.

I need take no notice of diallage rock, or of the more ancient red sandstone; as the same processes of reasoning apply to them as to those rocks to which they are analogous; but hornblende schist requires a particular consideration. This is an extremely fusible compound, and its peculiar crystalline texture proves that it could not have been deposited from water; in which, indeed, its earths are insoluble, and from which they could not thus have been precipitated. It is, besides, precisely analogous to many greenstones of the trap family; from which, indeed, it is often so little distinguishable, that it has been confounded with them, by those who choose to believe in the aqueous origin of trap, under the name of primitive greenstone. That it is further actually produced by heat, is evinced by finding that the argillaceous schists, when in contact with granite, are actually converted into it. Whether simple, or compounded of hornblende and felspar, the same reasoning applies to it. It is, nevertheless, admitted, that its original materials have been deposited from water, and thus its laminar and stratified disposition is explained. That it has further consisted of clay or schist, is not only rendered probable by the numerous facts occurring in the trap rocks, but by

that very striking analogy, now at last so well understood, in which beds of shale beneath trap are actually converted into Lydian stone; a substance differing from it, almost solely in the compactness and uniformity of its texture.

We thus lastly arrive at gneiss; a rock which often bears the marks of igneous consolidation in a still greater degree than those of aqueous deposition, but in which it is almost unquestionable that both have been combined. Where gneiss is at a distance from granite, its laminar and stratified disposition is most perfect; where in its vicinity, it is most obscure; indeed, so obscure, as at length to disappear. This is precisely what might be expected to happen on this view of its double origin; namely, the application of heat in unequal degrees, to a series of beds deposited from water, and, probably, like quartz rock, originally consolidated from it also. Where it is most remote from granite, although its mineral materials should be the same, they are disposed in a different manner; or are more rigidly laminar and more independent. Where it is most immediately in the vicinity of that rock, and more particularly when it abounds in granite veins, the structure becomes analogous to that of granite, or to one in which there is that mutual penetration of crystals which can only take place in a fluid of fusion. At length it actually passes into the contiguous granite; losing that parallelism of the parts, and those last remains of the laminar disposition, which had gradually been decreasing.

It is by no means difficult to imagine this combination of causes and of effects; a state of softening or semifusion, sufficient to allow the integrant parts of a stratified watery deposit to enter into new combinations, and to crystallize without the loss of the original marks of stratification. These, indeed, are often preserved in gneiss, by the alternate interposition of beds and laminæ of hornblende, and by that only; just as in the watery joint deposit of sandstone and shale, the latter substance is often the only indication of stratification that can be procured.

That such re-crystallization can take place in a rock which is heated to a point short of actual fluidity, is proved by Mr. Watt's

experiments, so often quoted; and that strata can, in nature, lose all their indications of watery deposition, while they preserve the stratified shape under a new mineral form, is evinced by the existence of siliceous schists beneath trap, as already quoted. A greater degree of heat and a longer continuance of it, are all that are required to produce all the differences in these cases; and the fact, of the frequent interposition of hornblende schist between beds of gneiss, is strongly confirmatory of the consistency and truth of these views. Thus, also, the transition of gneiss into granite becomes a phenomenon of easy solution.

Of the General Causes of Consolidation.

I need not here terminate this view of the consolidation of these primary rocks, by any general inquiries respecting the origin of the heat or its diffusion. Nothing can be said on this subject that has not been often said; and whatever difficulties may occur in attempting to apply these principles rigidly to every case that may be examined, it can only be said that this theory offers a general and obvious solution of the facts; and that if it cannot be exactly fitted to meet every exigency, it is no more than must happen in every similar case of a general principle, when we are not in possession of all the collateral circumstances by which it may have been modified.

In thus deducing, both from the agency of heat and of watery solution, the consolidation of all the stratified rocks, and in limiting these according to the various circumstances that have been indicated, it must be apparent that the power granted to the former is comparatively small, and that it is not here supposed to have acted beyond the range of the more ancient rocks, probably not through the whole of these. It is very possible, nevertheless, that the action of heat may have been much more extensive. But that it has acted in the consolidation of the secondary strata at large, is rendered in the highest degree improbable, by a variety of circumstances which I need not enumerate, because they have frequently been urged against the whole theory, to which the name of a party has been given

This is one of the unfortunate results that sometimes follows from attempting to prove too much; from overstraining an argument, so as to give advantages to the adversary, who, in finding a weak point, imagines that one blow will slay his enemy. It is not very good philosophy to disregard an obvious cause for the purpose of adopting a possible one; and it is a subject for regret, that those to whom geology is indebted for many rational views have too often exposed themselves to this censure.

But, in admitting that the great mass of secondary strata have been consolidated by a watery agent, it must be remembered that there is a wide difference between the consolidation and the precipitation of the same substances from water. If every one of these rocks did not give the most unquestionable proofs of its having originated, either in the ruins of more ancient rocks, or in the spoils of animals, it would be a sufficient argument against precipitation from a watery solution, that it involves every species of chemical and mechanical impossibility that can be included in a proposition so simple. It is unnecessary at present to detain the reader a moment longer on an hypothesis that would create and destroy oceans at its pleasure, yet find them ineffectual.

No notice has yet been taken of the power of mere pressure, either in actually consolidating rocks, or in assisting their consolidation. Yet it is an agent not to be overlooked; and when we consider the enormous weight to which the strata must have been subjected, it is very easy to conceive that its power cannot always have been inefficient. The occasional compression and fracture of imbedded shells, proves that it has sometimes acted; and if even the most delicate of these bodies are generally preserved, it only proves that they were well supported by the surrounding materials, not that they have not been subjected to great pressure. In our own experiments, with forces far inferior, clay can be compressed into a substance as hard as shale; and there are many of the schists not so hard as the heterogeneous mixture that is forced into a rocket, although composed of materials from which such an effect could scarcely be anticipated.

[This Paper will be concluded in our next Number.]

Art. VI.—*On Light and Heat from Terrestrial Sources. By* Baden Powell, M.A., F.R.S.

[Communicated by the Author.]

To the Editor *of the* Quarterly Journal *of* Science, *&c.*

Dear Sir,

Your readers will be aware from the reports of the proceedings of the Royal Society, that, on the 17th of February last, a communication from me was read on the subject of radiant heat; having been for some time engaged in the inquiry into the nature of the effects produced by the radiation from *luminous hot bodies*, as distinguished from that emanating from *non-luminous* sources, I have made many other investigations besides those contained in the paper alluded to. Some of these, comprised in the following remarks, may be considered as supplementary to the primary inquiry made in that paper; and they lead to a very simple theory of a subject which has hitherto been much involved in confusion. Should you favour me by inserting them in your Journal, I trust they may not be altogether uninteresting to your readers; especially as some of them are connected with topics which have formed the subject of some of your own experiments.

I remain, dear Sir,

Yours, very truly,

Baden Powell.

My whole inquiry is grounded upon the following assumptions, which, I conceive, are warranted by the most decisive experiments of Leslie, De la Roche, *&c.*

1. That simple radiant heat from *non-luminous sources*, produces its effect on bodies exposed to its influence in proportion to a certain peculiarity of *texture* in their surfaces, which is the same as that which gives them a greater power of radiating heat, and is altogether independent of colour.

2. That simple radiant heat is incapable of permeating glass

(at least of ordinary thickness,) however transparent. Whatever doubts may have been entertained on this point, I think are completely removed by the investigations of Dr. Brewster, in his paper on "New Properties of Heat," &c.—*Phil. Trans.* 1816, Part I., Prop. 40, &c.

3. That from *luminous* hot bodies a very considerable heating effect is produced, essentially different from that just described. It *passes through glass* by direct radiation, without heating it, and affects bodies in proportion to the *darkness of their colour*, without relation to the texture of their surfaces. This is evident from the results of M. De la Roche, (*Biot. Traité de Physique*, Vol. iv., p. 640, &c.) Mr. Brande, (*Phil. Trans.* 1820, Part I.); and other experiments.

4. That of the total heating effect from this class of bodies a considerable portion is stopped by glass. This appears from De la Roche's experiments; and further it is shewn that the degree in which this interception takes place, decreases in proportion as the body becomes more intensely luminous.

The theory adopted by Professor Leslie, as also apparently by M. Biot, in his account of the Relations of Light and Heat, (*Traité de Physique*, Vol. iv., p. 640, &c.), is, that this interception takes effect upon one simple agent, which is heat, more or less converted into light, according to the stage of combustion, &c., and is greater, as the agent approaches more nearly to the form of simple heat, in which case it is entirely stopped, or nearly so. In order to establish this theory, it would be necessary to shew, that whatever may be the particular law of relation to surfaces, by which the action of the "igneous fluid" is determined at any stage of its evolution, the portion intercepted should bear the same relation in this respect as the portion transmitted. At the higher degrees of incandescence, for example, this relation is to the darkness of colour. The same relation ought consequently to hold good with the portion intercepted, as with that transmitted by the glass. At lower stages, an approach to the preference for absorptive *texture* would be displayed, and this again should be equally found in both cases.

The experiments which I tried, were of a nature calculated to give an answer to this question, whatever result they should present. If they shewed the same relation maintained under the two different conditions, the theory of *one agent* would be established. If a different relation appeared, we must, of necessity, infer *two distinct radiations.*

In order to proceed with the following remarks, I must, of necessity, assume the result of these former experiments as established; but as it would be improper here to detail the contents of that paper, I shall merely state the general conclusion, which is, indeed, already before the public.

This conclusion, in fact, simply determines the question just proposed, in favour of *two distinct radiant agents,* or species of heating effect, which act jointly in the emanation from luminous hot bodies: of these, one is simple radiant heat, resembling in all its properties that from non-luminous bodies; that is, *stopped by glass,* and having relation to the *texture, not* the *colour,* of surfaces on which it acts: the other, a sort of effect associated in the closest manner with the rays of light, *passing* with them *through glass,* and affecting bodies in proportion to their darkness of *colour,* without respect to the texture of their surfaces. This last, for the sake of distinction, I call, "the heating power of light."

2. I may here premise a short account of one description of experiments, which I at first employed, in resolving this question, and which is not given in the paper alluded to.

These experiments were conducted upon the following principle, which, though very simple in theory, is in practice attended by several inconveniences, which, if not carefully guarded against, may lead to error.

A differential thermometer, having one bulb coated with smooth black, and the other with absorptive white, was exposed to the radiation from luminous hot bodies, having both bulbs at an equal distance from the body, first with, and then without the interposition of a glass screen. The arrangement is represented in figs. 1 and 2 of the annexed sketch. (Plate II.)

If the screen had no heating or cooling effect, it is evident that

in whatever proportion the radiant matter from any source affects the two bulbs, if that radiant matter be of one simple kind, the only difference on removing the screen will be, that the *intensity* of its action on the bulbs will be *increased;* but it will act on each in the same proportion as before; consequently an increase of effect, or a motion in the same direction as before, must, of necessity, take place. If, therefore, we perceive the reverse of this take place, it is a decisive proof that when the screen is removed, a *different agent* is brought into action.

But the effect of the screen will probably interfere to too great a degree to allow of this conclusion, without further precautions.

The influence of the screen will be reduced to nothing, if we can operate at a sufficient distance. Or again, we may judge of its effect by moving it nearer to the source of heat, when its cooling influence (if any) must be diminished. In observing the effect produced on the instrument during a short interval of time, 30 seconds for example, the effect of the screen must be of a *cooling* nature; and since, on the principle above stated, equality of distance in the bulbs is not an essential condition, we may place them obliquely, (fig. 3.) care being only taken that they remain exactly in the same condition when the screen is removed; in this way the difficulty is obviated. The black bulb being the worst radiator, by placing it nearest the screen, the cooling power will be displayed in the less apparent effect on this bulb; but if, in this position, an effect take place on this bulb from the transmitted portion of the radiation, it will be evident that on removing the screen, the effect, if due to one simple agent, ought to increase, from a twofold cause, the removal of the cooling mass, and the admission of a new portion of heat to the bulb.

In various trials of this sort, I never found an increase under these circumstances, and often a decrease; that is, the action on the other, or more absorptive, bulb was now increased; or the portion of the heat before intercepted has a different relation to surfaces from that transmitted. The effect was observed from incandescent metal, and from the flame of lamps and candles.

In these experiments, the effect is often very small; I have

placed no reliance upon them alone; and only mention them as additional variations, possessing considerable simplicity in their principle.

3. Having, as I conceive, in the investigation alluded to, established the distinct existence and joint operation of simple radiant heat, and the other heating agent, which for distinction I call the heating power of light, in the emanation from luminous hot bodies, it may not be uninteresting to examine the application of this doctrine to the results of former experimenters. In general it will be sufficiently obvious that the distinction thus established must apply to many well-known facts. Thus Mr. Brande in his paper on combustion, *&c.* (*Phil. Trans.* 1820, Part 1, Sect. 2,) states, that he obtained a considerable heating power on a blackened thermometer from the light of a flame of olefiant gas, collected by a lens, which did not become heated. This was evidently the effect of that portion of the radiant matter which is simply light possessing a power of communicating heat when absorbed: whilst all the other portion, namely the simple radiant heat was stopped by the glass; and was tending, if the experiment had been continued, to heat it.

I need not proceed to state the application of the same mode of explanation to various other results, as it must immediately occur, but there are several particulars respecting the ratio obtaining between the two parts of the total effect radiated from different sources, which it appeared to me very desirable to examine, in reference to the phenomena attending the evolution of radiant matter.

Upon the principle laid down, we may view the important results of M. De La Roche * from these: if we estimate the light by the effect through the glass screen, and the heat, by the total effect minus this last, we may obtain the following values for the ratio $\frac{l}{h}$ in the different cases:

$$\text{Iron at } 427^\circ \text{ centig. } \frac{l}{h} = \frac{1}{7} \text{ nearly.}$$

* *Biot. Traité de Physique*, Vol. IV. p. 640.

$$\text{Copper incandescent} \quad = \frac{1}{2.4}$$

$$\text{2d exp.} \quad = \frac{1}{2.8}$$

$$\text{Lamp, no chimney} \quad = \frac{1}{2}$$

$$\text{—— with chimney} \quad = \frac{1}{0.9}$$

In all the above instances, the value given to (l) should be slightly diminished for the heating effect of the screen. Also, the last ratio is of necessity inaccurate, because the simple heat was radiated from the glass chimney, and is probably greater than that which would be radiated directly from the flame. This ratio ought therefore to be increased. But even without this correction, it will hence be evident that in this series of luminous hot bodies, the heating power of light increases in a greater ratio than the simple radiant heat. And this increase of ratio corresponds to the degree of incandescence in the metal, and the completeness of combustion in the flame.

4. In order to prosecute this inquiry, I made use of the following application of the differential thermometer: if both the bulbs present vitreous surfaces, or are equally absorptive, the black bulb being affected by the light from a luminous hot body, and both bulbs being nearly equally affected by its simple heat, by interposing a small glass screen between the source of heat and the *plain* bulb, the effect of *both* is exhibited on the black bulb, or ($l+h$). Observing in the usual way we have the effect of the *light*, or (l). Hence we get (h) and the ratio of the two.

This arrangement is represented in Figs. 4 and 5. The influence of the two heating agents distinguished by the differently-dotted lines appears as in the former cases. Perhaps the numerical results obtained by such a mode of experimenting may not be susceptible of a great degree of precision, but for the purpose of ascertaining whether there was *an increase* of ratio or not, this

method is probably sufficient. In mentioning a few results which I have in this way obtained, I do not therefore conceive it necessary to go into numerical details.

5. It is well known that the general inferences made by De La Roche, with respect to the increasing *transmissibility* of heat in proportion to the degree of *luminosity*, have been confirmed by results of other experimenters on different principles, and in some instances the connexion between increase of *light*, and the peculiar *chemical* conditions of the case, have been established. By comparing such results, and viewing them on the principles now established, we get the connexion between those *chemical* conditions, and the increase in the *heating power of light*.

Thus with respect to an increased intensity of combustion, Count Rumford shewed (*Phil. Essays*, i, 304,) that in proportion to this increase the illuminating power increased. De La Roche has shewn that in the same proportion the transmissibility of heat is increased. Viewing this according to my principle, there is a corresponding increase in the ratio of the two radiations or $\frac{l}{h}$.

I have also confirmed this by applying the method above described (4) to the radiations from an Argand lamp, when its flame was in different stages of brightness; when a regular increase in the ratio was observable.

6. In the further investigation of this point, we may make some inferences from a consideration of Mr. Brande's experiments, (on gases, &c. *Phil. Trans.* 1820, Part 1.) He has there compared the heating effect by conduction of the flames of several species of gas; and has also estimated their relative illuminating effects. From the results given of the quantities of each gas requisite to produce equal lights and equal heats, we may deduce the proportions of the *effects* of heat and of light to be nearly as below, on the assumption that the heating is proportional to the illuminating power of light, and the radiant heat to the temperature in the different frames. Of the ratio in the same flame we can infer nothing.

GASES.	Ratios of light	heat
Olefiant	5.	2.5
Oil	1.9	1.5
Coal	1.	1.

Hence it would be obvious, that the heating power of light increases in a greater ratio than the radiation of simple heat in this series of flames.

If we compare these results with the remarks of Sir H. Davy, that the increase of light depends on the increase of solid product, it will be evident, from the chemical nature of the flames employed in this comparison, that not only the increase of light, but also the increase of *ratio* between the light and heat, takes place, in accordance with Sir H. Davy's doctrine. (See *Phil. Trans.* 1817. Part i. p. 75.)

7. But these inferences are grounded entirely on the assumption, that the heat radiating from different flames, is in a proportion not greater than that of the elevation of their temperature. This, perhaps, may not be much questioned. But the other part of the assumption, *viz.*, that the heating is proportional to the illuminating power of light from different flames, may admit of doubt. Thus, it would become very desirable to extend the examination, on the principle here suggested, to the flames of different gases. This part of the inquiry I have not the means of attempting; but there are other phenomena of a description closely analogous to these, and which, coming within the reach of familiar examination, I have made the subject of experiment, so, as in some degree, to supply the deficiency.

8. One case regards the alteration which takes place in a flame, as exhibited in the simple experiment of placing salt in the wick of a spirit lamp: the effect being increased by, at the same time, diluting the spirits with water. (See *Dr. Brewster's paper. Edinb.*

Phil. Journ. No. xix. p. 123.) This experiment gave a corresponding increase of ratio when the density of the flame was thus increased.

Another very striking instance is found in the phenomenon observed by Count Rumford and Mr. Brande, that the quantity of light is increased by placing several flames near each other, or so as to coalesce. Count Rumford considered the cause of this to be that the flames thus communicated heat to each other, or, in other words, that a portion of the heat otherwise radiated away, was retained. This then, it would seem, is engaged in some way in the greater extrication of light. To this phenomenon I applied the same method of observation, and found the ratio increase as it should do, if we could assume that the heat radiated increases in proportion to the temperature of the flame. For Count Rumford found the light to increase in much greater proportion than the temperature of the flame.

I found, for example, that when two flames of wax candles were made nearly to coalesce, the heating power of the light was considerably more than double that from one of the flames, whilst the radiant heat was less than doubled.

A series of trials with incandescent iron, gave a decrease of ratio corresponding to the degree of cooling, till the mass ceased to be luminous.

9. In Mr. Brande's paper, before referred to, one of the most interesting and important results, is the fact, that the chemical power possessed by the solar rays is not found in any sort of terrestrial light, except that produced from intense galvanic action. This would seem to indicate that galvanic light forms a term in the series, approaching more nearly to the solar light, than the most intensely luminous flame; and, since the law of inverse proportionality between the two radiations continues through all the instances of combustion, and is again exhibited in the solar rays when the proportion of the radiation of simple heat has become insensible, it is most probable, that if the galvanic radiation were examined by the method above proposed, we should find, corresponding to its resemblance, in chemical power, an increase in the

ratio of the heating power of light, to the simple heat, considerably beyond what is found in the most intense flames. This experiment I have not had an opportunity of trying: it is to be hoped this notice may lead to its being tried. And, perhaps, the further investigation of this point may promote some advance towards a knowledge of the nature of the chemical influence accompanying light under these circumstances. A power, of which, at present, we can only say that it is exerted by the solar rays *where no radiation of simple heat* is present; and in the greatest degree by those rays which have the *least heating power.*

10. It forms one of the most interesting topics of inquiry to examine the nature of the solar heat. A variety of experiments have long convinced me, that this heat consists *solely* of that kind which belongs to light. Among other modes of trial, I have often applied that here employed: which though not of itself sufficient to *establish* the two radiations, will yet infallibly shew whether there be present the smallest radiation of *non-transmissible heat.* With an instrument graduated according to Professor Leslie's scale, a variation of the 20th of a centigrade degree may be distinguished. I have repeatedly tried the experiment with the screened bulb, both plain and coated with whitewash, or with white silk. The screen could here be placed at a sufficient distance to preclude all interference. And, in these cases, after waiting till the instrument placed in the sun had become perfectly stationary, I never perceived the slightest increase of effect on interposing the screen.

11. These experiments afford us a point of comparison between solar and terrestrial heat. The former resembles, in all its properties, one species of the latter. But this is always accompanied by another species totally distinct.

With respect to the nature of the former, or the heating power of light, various opinions have been held. And it is to an examination of these opinions, and of the conclusions which we may deduce on the subject, from the experiments here described, that the remaining portion of my remarks will be devoted.

With respect to the heating power of the solar light, we have not at present many data from which we can deduce a view of its

nature, otherwise than by analogy with the similar power possessed by terrestrial light. This we can trace to its source, and may thus be enabled to form opinions with tolerable certainty as to its nature.

12. Philosophers have been much divided in their opinions respecting the nature of the relation thus subsisting between light and heat. One party have maintained the absolute identity of those agents; accounting for the different properties exhibited by this heat, and by simple heat, only by supposing some modifications to take place in the state or form in which the "igneous fluid" exists, and by which it becomes either light or heat, according to circumstances; each possessing many of the properties of the other. Another opinion has been that of the totally distinct existence of the two; although they accompany each other in the closest and apparently most inseparable state of connexion.

The former of these opinions appears to me little more than a gratuitous assumption; and the latter is attended with insurmountable difficulties, if we suppose the heat so accompanying light to retain its separate existence and radiant properties. These, in fact, are entirely changed.

13. It appears from the most decisive experiments, that the sun's rays have a power of producing *heat* in bodies in proportion to the degree in which their surfaces (according to the common expression) *absorb light*, from their darkness of colour.

The cause to which this effect is to be attributed, whatever may be its nature, is clearly shewn to be so closely associated with the rays of light, that it seems impossible, under any ordinary circumstances at least, to effect a separation between them. It is transmitted with light through transparent bodies; increases or decreases with the intensity of light; and as nearly in exact proportion as the nature of the appropriate experiments will allow us to ascertain. Professor Leslie considers the proportion to be precise and undeviating: it is refracted with light to a focus; and communicates no heat to transparent media through which it passes; it is reflected with the rays of light, and polarized with them. It is also, as is well known, subjected to a peculiar distri-

bution among the primary rays when analyzed by a prism. But the conclusion once maintained of an absolute separation of heating rays by this means, has been more recently shewn to be most probably owing to red rays of so deep a colour as not to be ordinarily visible. (See *Mr. Herschel's Paper, Edinb. Trans.* 1823, §. 7.)

And though the recent experiments of Dr. Seebeck have shewn that this heat is distributed to different parts of the spectrum in very different degrees, according to the different nature of the prism employed, yet since the light is known to be also similarly affected, this is no proof of the distinct existence of separate rays of heat.

Upon the whole, then, we must view these effects as due to a certain power or property of communicating heat belonging to the rays of light. But this need by no means involve the supposition that light and heat are identical, or can be converted the one into the other. But with respect to this view of the subject, we may obtain greater certainty by returning to the subject of the foregoing experiments.

14. At the commencement of this inquiry I viewed it as bearing upon the theory, which asserts that the radiant matter from luminous sources is of one species, only gradually changed from heat into light. The facts here established so far disprove this opinion, that we evidently perceive a very considerable portion of the radiant matter undergoing no change whatever, except an increase in intensity.

If, therefore, we still adhere to the supposition that light is only heat in a different state, we must so far modify the hypothesis as to admit that only a part of the igneous fluid undergoes this change.

15. But without assuming any hypothesis, it is evident that the total effect, whether communicated directly, or by means of the light, must have originated in some way from the hot body; and although I have proved the total effect to be distinguishable into two parts or species, we still cannot consider them otherwise than as both derived from the same source. I conceive it already suf-

ficiently established that a portion of the heat of the radiating body actually disappears, (at least in the form of radiant heat,) or is in some way totally changed in its properties: yet it may be worth while to observe, that the experiments now recorded, or quoted, afford a more palpable confirmation of this conclusion.

16. When we consider that in every part of a flame an intense chemical action is going on, and a high temperature in consequence generated, it will follow that if by any external means the intensity of that action is increased, a proportional quantity of *heat* must be generated, and the intensity of such action is increased in the instance of a more complete combustion being produced by external means. That the quantity of light evolved, and perhaps also its intrinsic heating power, undergo an increase, whilst the simple heat does not increase so fast, shews that of the increased degree of heat generated by the more complete combustion, an increasing portion is occupied in the production of light.

17. The same truth is exemplified with additional force in the instance of a flame whose light is increased by the greater evolution of solid ignited particles, whilst its radiant heat does not sustain a proportional increase. If a particle of solid matter be volatilized into a flame of gas, the temperature of the gas is communicated to it; and the same temperature makes the solid particle give out much more light (estimated by its heating effect) than the gas, but not as much more heat. Therefore a portion of the heat communicated to the solid particle disappears as radiant heat, and is occupied in the evolution of light.

18. In the experiments on uniting different flames, the same thing is exhibited in a still more palpable manner. We there perceive that the simple heat radiated from two flames united, is much less than double that radiated from one. But yet, since by the junction of two equal masses of luminous matter at equal temperatures the heat must be doubled, it is evident that a portion of it is abstracted, and that at the expense of this portion the heating power of the light is increased.

We might argue in the same way from the experiments on incandescent metal. Since the total effect, or the values of $(l + h)$

must follow, (allowing for the nature of the determinations,) the same law as that of the cooling of the hot body, since those of (*h*) follow a less rapid law, we might infer that part of the heat was constantly abstracted, or ceased to appear in the form of heat; and this in proportion to the increased heating power of the light; the greater evolution of which thus contributes to the cooling process.

19. If it be admitted, as I conceive has been before shewn, that the whole heating energy of a luminous hot body is not displayed by the simple heat radiating from it; but though partly thus displayed, is also partly as it were communicated to another agent, through the medium of which it ultimately acts; this conclusion will receive still further confirmation from the arguments just adduced. From them we perceive that a continued and increasing disappearance of simple radiant heat always accompanies the increasing developement of heating power in the luminous rays.

20. I have adverted to the difficulties attending the supposition that the portion of heat which is not radiated in its simple form is *converted into light;* but since it is evidently abstracted, and afterwards appears again through a different channel, it will hardly be questioned that it is in some way employed in the extrication of the light. It, therefore, becomes important to inquire (so far as we can ascertain,) what is the real modification which it undergoes.

The facts teach us thus much:—This portion of the heat acquires totally different properties from those which it possessed either when in combination with the body from which it emanates, or which the radiated portion possesses. Though we have no right to *identify* it with light, it is yet evident that it is in some very close state of *union* with the light. It is so modified as to have lost its power of heating many sorts of solid matter; *transparent* bodies for instance; through which it is as it were conveyed, without any developement of its power of increasing temperature. Again it is not upon *all opaque* bodies that it can now exert its influence; and the degree in which that influence is exerted depends upon quite different characteristics in such bodies from those by which its ordinary influence is most developed. It accompanies the rays of light in their course, however altered;

and is never rendered sensible but when the light itself undergoes some change in its state. It disappears when light is formed or given off from bodies; and re-appears when light is absorbed, or enters into combination. The principal part, at least, of the phenomena cannot be described as belonging to what we might call the *temperature* of light; for then a black and a white surface would be equally heated by its impact upon them.

21. The relations of light and heat have justly been considered as involved in much obscurity; but I cannot help thinking that the difficulties of the subject have been regarded as greater than they really are.

Of the nature of these two grand agents in physical phenomena we are completely ignorant, that is, we have not been able to detect any certain particulars in which they can be reduced by analogy to their place in the arrangement of bodies: from this cause it seems to have been tacitly taken for granted that since their *nature* is probably altogether *sui generis*, so their *mode of action* upon, or *connexion* with each other must be also equally beyond the dominion of ordinary natural laws.

To this inference, however, I think we are far from being necessarily led. With the powers of *light* as a chemical or physical agent upon the bodies, we are but little acquainted; all that we know may be reduced to a few insulated facts. On the other hand, with respect to *heat*, the case is widely different; not only do we recognise its action more or less in almost every phenomenon which nature presents, but nearly all the instances of its action have been reduced to general laws, and explained on regular theories.

Hence in attempting to investigate the relations subsisting between these two agents, it appears to me the course most likely to afford satisfactory information, that we should in the first instance take that of the two with whose effects we are best acquainted, and observing the laws of its connexion with ordinary matter, inquire whether any of those laws will apply to its connexion with light: this would be the course which on sound principles we ought surely to prefer before that of framing new suppositions to

account for effects which we suppose to be *sui generis*, only because we have not examined whether they are in accordance with any other class of phenomena.

22. What then are the most proper terms in which to describe the facts?

The phenomena which light exhibits in its relations to heat, agree in the closest conformity with those presented by the changes of the ordinary forms of matter. When light is absorbed and enters into combination with other matter, heat is given out; on the other hand, light is not generated or evolved without the application of a certain degree of heat. All bodies at some temperature become luminous; and when they arrive at that point, a portion of the heat is employed in giving the form of light to some matter belonging to, or in combination with, the body *by becoming latent in it.*

23. To this view of the subject, I conceive we are directly, led by the foregoing experiments: the different results all afford a strong corroboration of each other; and from them it appears, that instead of the vague opinion that "heat and light mutually evolve each other," we have it in our power to explain the phenomena which their union and separation present, in a way equally simple and satisfactory, and agreeing in the closest analogy with other physical phenomena; and when the terms "absorption of latent heat," *&c.*, are carefully used in their strict experimental sense, it is obvious that we do not, in applying them, necessarily assume the materiality of light, or of heat; though, perhaps, the facts here brought forward may be considered as new arguments for it.

It would lead into details of too great length here, to proceed to the application of the principle above inferred, and to point out the ready explanation of many phenomena which it affords, and which, upon the received theories, are allowed to involve great difficulties. But it may be proper briefly to mention one or two instances.

24. The light from phosphorescent bodies is too feeble to allow us to ascertain whether it possesses any heating power when absorbed. In the absence of proof, analogy would lead us to suppose that

such a power must exist, though probably of very small intensity; and the phenomena which these substances present, on the principles here advanced, agree in the closest analogy with other phenomena of nature. Some bodies, as water, mercury, &c., change into elastic fluids at common temperatures; in the same way the common temperature of phosphorescent bodies may be sufficient to afford to some of their peculiarly-constituted particles, the requisite degree of latent heat, to evolve them in the form of light.

25. Phosphori, when exposed to light, absorb it, by which some degree of heat is necessarily communicated to them. This is in part occupied in causing the light to be again evolved, by becoming latent in it, as soon as the body is removed from exposure to light. An increase of temperature accelerates this process, and too great an increase soon exhausts the supply of luminifiable particles.

26. In general, I may further observe, it is on these principles easy to conceive, that there may be cases in which, for the evolution of light, no increase of sensible temperature should be necessary. The whole of such increase, though really generated, becoming latent in the light.

In general, also, the heating power of light evolved from different bodies, need not follow any proportion to its illuminating effect. It is conceivable that a body may produce light containing any given degree of latent heat, but of such tenuity, or so deep in colour, as to have very little illuminating intensity: of this we have seen an instance in incandescent metal.

I might proceed to the application of the principle here adverted to, in a variety of other cases; but upon these topics it will not be necessary to go any farther. I will merely observe, in conclusion, that as Newton has put the query, whether light and common matter be not convertible into each other, we here perceive phenomena which render it highly probable that such a change actually does take place; since, whatever the change be, it is accompanied by precisely the same phenomena in regard to latent heat, as those by which the changes of state, in ordinary matter, are accompanied.

Art. VII. *On Anhydrous Sulphuric Acid.* By Andrew Ure, M.D., F.R.S., *&c.*

Dear Sir,

Several years ago I procured from a friend at Hamburgh a bottle of the oil of vitriol of Nordhausen, intending at that time to examine its constitution, and to give you an account of it, in supplement to my paper on sulphuric acid, inserted in the 7th number of your Journal. This bottle was by accident overlooked by me till lately, when I made the following experiments on its contents.

A portion of this brownish-coloured oil of vitriol, sp. grav. 1.842, was distilled slowly from a glass retort, into a globular glass receiver, surrounded with ice. A white sublimate soon appeared in the globe and beak of the retort. When this sublimate was exposed to the air, it emitted a profusion of dense fumes of sulphuric (not sulphurous) acid. It burned holes in paper, with the rapidity of a red hot iron.

A phial, containing 988 grains of distilled water, which occupied two-thirds of its capacity, was poised in a sensible balance. Into this water a piece of the acid sublimate, of a tallowy consistence, slightly deliquesced on the surface, was dropped, and the phial was suddenly closed with its stopper, to prevent the ejection of liquid by the explosive ebullition which always ensues, when this solid acid comes into contact with water. The increase of weight due to the introduction of the acid into the phial, was found to be 56 grains. The total weight and specific gravity of this dilute acid were at 60° Fahr. 1043.6 grains, water being 1000. At this density, 1000 parts of sulphuric acid, of the common kind, contain 53 parts of dry acid, equivalent to 65 of liquid acid, sp. gr. 1.844. But in 1000 grains of the above dilute Nordhausen acid, there was at the utmost only 53.6 grains of the sublimate, which, considering the deliquescence on its surface, is a quantity agreeing with the supposition of its being anhydrous acid.

To put this point more fully at rest, I saturated 987 grains of the above dilute acid with pure carbonate of potash, prepared from

tartar, of which 90 grains were required. These 90 grains are equivalent to 51.5 grains of dry sulphuric acid. Now 987 grains of the dilute acid of Nordhausen, contained of the solid sublimate 52.9 grains, giving an excess of weight = 1.4 gr., due, most probably, to the adherence of moisture to the surface of the sublimate: 112 grains of dry sulphate of potash were obtained by evaporating the above saline solution. This portion of ignited salt contains 51 grains of dry sulphuric acid.

From the preceding experiments, it seems likely that the white sublimate from the Nordhausen oil of vitriol is anhydrous sulphuric acid.

I am, dear Sir,

Yours, &c.

Glasgow, March 1st, 1825. ANDREW URE.

ART. VIII.—*Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by* William Thomas Brande, F.R.S., Professor of Chemistry in the Royal Institution, *&c.*

[This abstract of the above Lectures is published at the request of several of the Members of, and Subscribers to, the Royal Institution; they are intended to shew the objects of Geological Science, and to furnish the student with an outline which may assist his progress in the pursuit of the study. The general arrangement of the subject nearly corresponds with that adopted by Messrs. Conybeare and Phillips, in the first part of their excellent work on the Geology of England and Wales; from which, and from the invaluable collection of facts contained in the Geological Transactions, ample abstracts will be found in the following pages. To these works the geological student is especially referred, for such details as were inconsistent with the plan of a popular course of lectures upon so extended a subject.]

I SHALL endeavour, in this preliminary discourse, to give a very brief outline of the origin and progress of geological science; to explain particularly the mode of pursuing it which it is pro-

posed, upon the present occasion, to adopt; to shew the interest and the usefulness of the study in its various applications; as illustrating the natural history of our planet; as unfolding those adjustments of inanimate nature which are calculated to display the wisdom of the creation; as leading us to results useful in the arts of life; and as propounding to the inquisitive mind an infinite variety of questions and speculations connected with the causes of the effects which we now perceive; with the events which they announce as having happened at remote and obscure periods of the history of the Earth; and with the various revolutions and changes which our globe seems destined to undergo, by the continued operation of the powers now active; and by that perpetual warfare of the elements to which its surface is continually submitted. The bare mention of these, the genuine and legitimate objects of Geological science, naturally brings to the mind the awful and magnificent account of the creation, conveyed to us in scriptural history; and geological writers have not unfrequently attempted to combine their speculations with the announcements of holy writ.—Mixing up the chronology of Moses and the history of the deluge with their own short-sighted speculations, and with observations hastily made and imperfectly reasoned upon, they have presumed, on the one hand, to verify and illustrate, and on the other, to question and controvert. But the arrogance of imperfect knowledge is nearly equally prevalent in both; "nothing," says Lord Bacon, "is more pernicious than to canonize error:" and again, adverting to the blending of natural philosophy with sacred writ, he calls it "seeking the dead among the living," and justly observes, that "such vanity is so much the rather to be restrained and suppressed, as from the wild mixture of divine things with human, [illegible] not only fantastical philosophies, but heretical religi[illegible] Far, therefore, from endeavouring to explain or controvert the arguments which have thus been by some annexed to, and blended with, geology, I shall altogether omit them, referring such as are interested in the legitimate part of the discussion to the masterly work of Mr. Granville Penn, entitled "a Com-

parative Estimate of the Mineral and Mosaical Geologies,*" and I shall almost exclusively confine myself to the detail of those facts which lead to useful conclusions; hypotheses I shall not enlarge upon, because our time is very limited, and they rather amuse than instruct; and I shall only lightly touch upon such theories as are remarkable for their notoriety, or important from their connexion with, and illumination by, the leading facts of our science: but upon this subject, I propose to explain myself more fully in another lecture.

Geological writers may be divided, into those who are purely speculative; those who have built theories upon the examination of the structure of the earth's surface, or, at least, profess to do so; and those who, discarding speculation and theory, have contented themselves with the abstract detail of facts.

Of the former class, Dr. Thomas Burnet, who must not, as he sometimes has been, be confounded with the celebrated Bishop of Salisbury, with whom he was contemporary, stands pre-eminent. This writer, in his Sacred Theory of the Earth, which was originally published in Latin, between 1680 and 1690, and translated into English at the express request of Charles II., and which has been extolled for its eloquence and ingenuity by many of the most eminent authors, has taken a review of the past changes of the globe, contrasts them with those it is now undergoing, and foretells those which it is to suffer; and as the name of Burnet is continually occurring in geological history, it will not, I trust, be thought irrelevant, briefly to enumerate his opinions, more especially as this is the only time that I shall mention him or his doctrines.

In the first place, he ransacks scriptural and profane history, selecting from each such statements as suit his particular object, and endeavours to show that the primeval earth, as it arose out of elementary chaos, was of a form and structure different from that which it now exhibits, and so contrived as to contain within itself the materials necessary to the production of an

* See, also, a Review of Mr. Penn's work, in the fifteenth volume of this Journal.

universal deluge. He tells us, that when the elements separated from the original fluid mass, the heaviest particles tending to a centre, constituted a nucleus upon which water and air afterwards assumed their respective stations. The air, however, was not as we now see it a transparent attenuated medium, but it was loaded with exhalations and impurities which it gradually let fall upon the surface of the water, and then floated upon the whole in cloudless serenity. The deposited matter, constituting a rich crust, sent forth its vegetable productions, and soon became clothed with uninterrupted verdure; every thing was smooth, soft, and regular, and there was, he says, an universal spring; for the plane of the ecliptic was coincident with that of the equator. In process of time, however, the green and even surface just described, began to suffer from the continuous action of the sun's rays, which formed cracks and fissures that ultimately extended to the abyss of waters beneath, and these being sent forth by elastic vapours expanded by heat, soon inundated the superficies; an universal deluge ensued; and, in the violent shocks and concussions that attended it, rocks and mountains and all the inequalities of the present surface had their origin; then the waters gradually subsided into the residuary cavities forming the ocean, and partly were absorbed into the crevices of the disjointed strata and nucleus; vegetation began to re-appear, and the once uninterrupted and uniform surface was now broken up into islands and continents, and mountains and valleys.—Absurd, as from this condensed and unadorned sketch, Burnet's narrative must appear, it is told with such ingenuity and elegance, and supported with so much erudition, as to entitle it to all the merit that can belong to a highly elaborate and poetical fiction. Addison has eulogized it in Latin verse, Steele has praised it in the Spectator, and Warton, in his essay on Pope, ranks the author "with the select few in whom are united the great faculties of the understanding; judgment, imagination, memory."

But, although Burnet received and deserved the encomiums of the learned, the praise that he earned is rather that of the poet than of the philosopher. Dr. Flamstead, adverting to his rich

vein of poetical diction, told him "that there went more to the making of a world than a well-turned period;" and Mr. Warren, and Dr. Keill, of Oxford, each refuted and abused him as a theorist. Yet Burnet's work continued to be read, not for its philosophic truths or theoretic consistency, but for its splendid imagery, noble sentiments, and sublime conceptions.

Passing over several speculative authors, who, compared with Burnet, are neither instructive nor amusing, we meet in geological history with the writings of Woodward, who flourished in the latter half of the 17th century, and who must, I think, be considered as the earliest geological theorist who has professed minutely to examine the earth's external crust, and to found his opinions upon the results of its detailed inspection. In visiting the country about Sherborne, in Gloucestershire, he was struck with the variety of shells and marine productions visible in the strata; and he determined, with a degree of zeal which was then not very common, to undertake a geological tour, with a view of examining how far similar appearances were to be found in other quarries, and in remote parts of the kingdom. Having satisfied himself upon these subjects, and after registering in his journal a very copious account of his observations, he drew up a series of queries which he distributed amongst his friends and correspondents abroad; and, as the result of all his inquiries, he concluded that the earth's structure was not materially different in any part of the world, but that a general resemblance pervaded the contents and positions of its various beds and strata. In 1695, he published a work entitled "An essay towards a natural history of the earth and terrestrial bodies, especially minerals; as also of the sea, rivers, and springs. With an account of the universal deluge, and of the effects it had upon the Earth." This essay which is scarcely so much known as it deserves, excited a good deal of bustle amongst the philosophers of the period in which it was written; it was attacked, canvassed, examined, and defended, and called forth all those ephemeral answers, replies, and rejoinders, which flutter about controversy. Woodward did not confine himself to geology,

he attracted some notice as a physician, and more as an antiquary; and in his last will he founded a lectureship in the University of Cambridge; he died in 1728.

When we consider the untoward circumstances of Dr. Woodward's education, and the obstacles that in early life were opposed to the natural bent of his genius or inclination, we must allow him no small merit in encountering and overcoming them. At the same time, his life and writings are a good deal sullied by a peevish jealousy and visionary enthusiasm. He is ridiculed by Pope, under the name of Vadius, in his *Moral Essays;* and again in several parts of the *Memoirs of Scriblerus*, where an ancient shield, which the Doctor possessed, becomes the chief subject of the poet's merriment.

Among the correspondents and opponents of Woodward we meet with several authors whose works are never read, and whose names are falling fast into entire oblivion; there were others of more celebrated memory, and among them Leibnitz, who, towards the end of the 17th century, published his *Protogæa*, in which there is little more than crude and improbable speculations relating to the agency of fire upon a supposed chaotic mass. Nor are the geological opinions of Whiston deserving of more attention, though his work, published in 1696, entitled "A New Theory of the Earth, wherein the Creation of the World in Six Days, the Universal Deluge, and the General Conflagration, as laid down in the Holy Scriptures, are shown to be perfectly agreeable to Reason and to Philosophy;" gained him great notoriety.

It would, however, be an injustice to Whiston, were I to pass him by without quoting Locke's eulogium of his Theory of the Earth; who says, in a letter to Mr. Molyneux, bearing date Feb. 22, 1696:—"I have not heard any one of my acquaintance speak of it but with great commendations, and, as I think, it deserves: and, truly, it is more to be admired that he has laid down an hypothesis whereby he has explained so many wonderful and before inexplicable things, in the great changes of this globe, than that some of them should not easily go down with some men, when the whole was entirely new to all. He is one of those sort

of writers, that I always fancy should be most esteemed and encouraged: I am always for the builders who bring some addition to our knowledge, or, at least, some new things to our thoughts." But Locke's opinion upon such a subject, is not entitled to any especial weight. I have perused Whiston's book, without being able to find any particular merit in his speculations, and am rather inclined to side with his opponents, in forming an estimate of its value.

About this time a central fire was resorted to for the purpose of solving certain geological problems, by so many writers, that it is difficult to say to whom the merit, if such it be, of the invention belongs. To the views founded upon such a supposition, I shall advert more fully in another lecture; Beccher's *Physica Subterranea* deserves especially to be consulted in reference to this subject.

About the middle of the last century, geology began to assume a more regular scientific aspect; and among the earliest inquirers of this period, Mitchell and Whitehurst deserve something more than bare mention. The merits of the former writer have been overlooked, principally, I presume, on account of the title of his paper, which is in the *Phil. Trans.* for 1760, "On the Cause and Phenomena of Earthquakes," a title under which we should not perhaps look for geological, and still less for minute, practical information. We are chiefly indebted to Dr. Fitton for bringing him into notice, in an able article on English Geology, contained in the *Edinburgh Review* for 1811. This very ingenious writer describes the general appearances of the strata, points out their analogies and differences, adverts to their inclination and disturbance in mountainous districts, and to their horizontality in flat countries; and, having explained, with much minute and practical perspicuity, the arrangement of the strata in England, he exemplifies its universal application to the general structure of the globe; and ingeniously represents it in the following manner. "Let a number of leaves of paper," he says, "of several different colours, be pasted one on another; then, bending them up together into a ridge in the middle, conceive them to be reduced again to a level surface by a plane so passing through them as to cut off

all the part that had been raised; let the middle now be again raised a little, and this would be a good general representation of most, if not all, large tracts of mountainous countries, together with the parts adjacent, throughout the whole world. From this formation of the earth it will follow, that we ought to meet with the same kinds of earths, stones, and minerals, appearing at the surface in long narrow slips, and lying parallel to the greatest rise of any large ridge of mountains; and so, in fact, we find them.

Mr. Mitchell's paper abounds in important geological generalizations, and he applies his theories and inquiries with much dexterity and success to the structure of the whole surface of the globe, as well as to its individual parts.

Whitehurst is another writer of great merit in the history of English geology: in early life he passed a great part of his time in Derbyshire, a county well suited to excite and satisfy a mind endowed with the desire of penetrating into the formation of rocks, and into the origin and history of organic remains. The fruit of these investigations he submitted to the world in 1778, in his "Inquiry into the Original State and Formation of the Earth;" a work which, in its general outline and particular execution, does no small credit to the genius and diligence of the author. It is true, that much of it is tinctured by that unpropitious taste for cosmogony, which we have reprobated in preceding writers; but if we look to the practical details, we find them faithful to nature, and described with correct minuteness. To this point I need only quote the following passages. "The arrangement of the strata," he says, "is such that they invariably follow each other as it were in alphabetical order, or as a series of numbers. I do not mean to insinuate that the strata are alike in all the different regions of the earth, with respect to thickness or quality, for experience shows the contrary; but that in each particular part, how much soever they may differ, yet they follow each other in a regular succession."

In the writings of Mitchell, and of Whitehurst, then, we begin to discern something like a genuine and scientific investigation

respecting the structure, position, and contents, of the strata that envelope our globe. I have said nothing of Buffon, whose theories and speculations are much of the same cast as those of Burnet; nor of Rouelle, who reasoned more correctly, and discriminated with greater judgment; wishing to confine myself chiefly to the opinions, as well as to the writings, of our own countrymen. But in the works of this period one very important fact must not be overlooked, relating to the distinction insisted upon chiefly by Lehman, and some other continental authors, which may be made between rocks containing organized fossils, and those which are destitute of such remains; the former bearing evident traces of great revolutions and changes, the latter, apparently of an anterior date, and exhibiting no marks of animal or vegetable relics: the former, in the words of Lehman, owing their formation to partial or local accidents and derangements, the latter coeval with the world. To these which have since been called *secondary* and *primary* rocks, he added a third class, in which, as to position and structure, he professed to recognise the operation of the deluge; he presumed that they must have resulted from some great catastrophe, tearing up and modifying an ancient order of things. But in respect to the classification of rocks, it will be our business to speak more at length hereafter: and apprehensive that I may already have fatigued your attention with matters rather curious than useful, and with details historical rather than practical, I shall only further trespass upon it by briefly adverting to the speculations of two comparatively modern theorists, whose opinions have divided the geologists of our own time, and who may be called the founders of its once opposed schools; premising, however, that in their arguments and hypotheses, as in those of their predecessors, we shall find much that is blameful and faulty, mixed up with a body of practical and matter-of-fact information, of infinite use and value: indeed, though it is unfortunately my business to adduce many of these theories rather to refute, than to elucidate them; and though they are, with few exceptions, so many imaginary fabrics, displaying rather the fol-

lies than the genius of the wise, it must be remembered that to them we chiefly owe the practical foundations of geology.

It would be easy to shew that the theories, or rather hypotheses, of which we have now taken notice, contain the germs of those speculations and inquiries which in our own days have excited so much attention and controversy under the name of PLUTONIAN and NEPTUNIAN doctrines. The Neptunists affecting to trace all the present appearances of the globe to the sole agency of aqueous solution, disintegration, and deposition; and the Plutonists denying the exclusive operation of water, but combining its powers with those of fire, and calling in the aid of both elements.

The credit, such as it is, of the Neptunian theory, is commonly given to Werner; and if we find in it all the faults of his predecessors, and all the erroneous reasoning of darker ages, it will, at the same time be recollected, that to him belongs the principal merit of pointing out the order of succession which the various natural families of rocks are generally found to present, and of having himself developed that order to a considerable extent with a degree of accuracy which before his time was unattainable, for want of proper methods of discriminating minerals and their compounds. There is one disadvantage and difficulty attending an attempt to expound Werner's doctrines, which is, that we are obliged to take them at second hand, since he has not published any connected view of them himself; and moreover, the Professor and his scholars have generally affected a mysterious phraseology which it is very difficult to construe into common sense, or intelligible terms; and which is sometimes so harsh and uncouth as to border upon the ridiculous, when, at least, we attempt to put it into an English dress. The darkness which he has thrown round his doctrines seems, indeed, often as if it were expressly intended to keep them from the eyes of the vulgar and uninitiated, and may be compared to that mystic and symbolical language in which the alchemists delighted to veil the accounts of their researches; and which after all were no great things when by dint of much labour and study they were deciphered and done into plain and legible language.

In speaking, therefore, of Werner's theory, we can only avail ourselves of such transient glimpses as he has himself thought fit to give us, and must fill up the various chasms and breaks, with materials derived from the more extended and finished sketches and illustrations with which we have been favoured by his pupils and disciples. Werner's theory then amounts to this:—The matter of our globe was once in a fluid state, or at least its nucleus was enveloped by a chaotic solution of such a nature as to retain the various earthy bodies found in the lowest strata in chemical combination; but this state of things was of short duration, and the chaos began to deposit a variety of crystalline aggregates, such as the different species of granite, certain kinds of slates, or as they are more technically called, schists; the genuine kinds of marble, serpentine, and porphyry, and a few other more equivocal compounds. These constitute the *primitive* rocks or formations of the Wernerian school; they are supposed to have had their origin antecedently to the creation of living beings; they are more or less crystalline in their texture, and never contain any organic remains or rounded pebbles.

The second class of rocks included in this arrangement, are supposed to have been formed during the transition of the Earth from its chaotic to its habitable state. They are partly crystalline aggregates, and partly mechanical deposits; they contain fragments of pre-existing rocks, and are sparingly interspersed with imperfect remains of some of the lower orders of animals; certain dark-grey compact limestones, and the rocks called grauwacke, composed of fragments imbedded in a slaty paste, are the leading members of this family.

It is, then, imagined, that the elements acting upon these older rocks tended to their attrition and disintegration, and that several substances being mechanically diffused throughout the waters that covered the primitive and transition series were deposited upon them in successive layers, in a horizontal position. These are Werner's Flœtz rocks; they not only contain, but even abound in, vegetable and animal remains; and among the latter, skeletons of amphibious animals are not uncommon.

Certain limestones, red sandstone, coal strata, lias, and chalk, and some other sandstones, belong to this division.

Lastly, we find depositions of sand, gravel, and clay, of the bones of quadrupeds, accumulations of peat, and some other substances now in progress of accumulation or deposition, which are included under the term alluvial formations.

The fifth class contains the produce of volcanic fires, and of more partial combustions. So that under one or other of these classes, or *formations*, as they are theoretically called, it is supposed that the various substances occurring amongst our rocks and strata may be included. It would be premature, to descant upon or criticise this theory, without more particular notice of the facts upon which it rests, and of those which militate against it, than I now can lay before you; but I think that you will hereafter, when in the possession of details, find me borne out in asserting, that it is, in all points, weak and unsatisfactory. That the very idea of an universal solvent, and of crystalline, succeeded by mechanical deposits, is at variance with all experience or analogy; in short, that it includes an unwarrantable accumulation of hypotheses, assigning opposite qualities to the same agent; and that, like most of its predecessors, it is equally at variance with nature, and with itself—"in a word, that it is a system which might pass for the invention of an age when sound philosophy had not as yet alighted on earth, nor taught man that he is but the minister and interpreter of nature, and can neither extend his power nor his knowledge a hair's breadth beyond his experience and observation of the present order of things."

The Plutonic theory, as it is generally though not quite properly termed, owes its origin to Dr. Hutton, and proceeds upon principles differing from those of the ancient Vulcanists, at the head of whom we may place Whitehurst, and entirely at variance with the leading tenets of the School of Freyburgh. It has been defended by the late Mr. Playfair, under the term of the *Huttonian Theory*, and his "Illustrations" rank among the most eloquent of scientific compositions; not but that he some-

times adduces doctrines which neither experiment nor analogy are competent to sanction, and which are rather adapted to delight the fancy than to convince the reason.

The Huttonian theory supposes the materials which compose the present surface of the globe to have been derived from the action of water upon a former order of things; that they are, in fact, the debris or ruin of ancient continents, which have been pulverized and worn away by the continuous operation of torrents and currents of water, which had transported them to the bottom of the ocean, and that there they have been consolidated by various causes, but chiefly by subterranean or volcanic fires; and we are to imagine the expansive power of the same irresistible agent to have again elevated the strata from the bottom of the ocean, to have given them various states of induration, and to have thrown them into those differing degrees of inclination to the horizon which they now exhibit; simply raising them in some instances; dislocating and removing them from their old posture in others; and occasionally effecting their entire fusion. The unstratified substances are supposed to have been in the latter predicament, while the stratified bodies are regarded as having been only softened by heat, or penetrated by melted matter. And as present continents were formed from the disintegration and corrosion of prior rocks, so are they supposed to be gradually restoring their materials to the sea, from which new continents are hereafter to emerge, manifesting a series of changes similar to the past.

Though in the details of these theoretical views there is very much that is fantastic and improbable, it must be allowed, that there is also much that is consistent with the known agency of bodies, and which is even directly borne out and verified by the actual result of experimental investigation. Indeed, the progress of modern chemistry has disburdened the Huttonian doctrines of some of their heaviest inconsistencies.

When facts and specimens are before you, I shall beg leave to call your attention to some of the chief elucidations of the Wernerian and Huttonian hypotheses; and then, and not till then,

shall we be able to make up our minds respecting their relative value and merits. I shall, however, dwell upon them, not so much from any conviction of their intrinsic importance, as with the intention of shewing the chief, and indeed real value of these and similar disquisitions, that of awakening in the mind a desire of investigating nature and of collecting facts: for observations accumulate very slowly when unassisted by the influence of system—the observer never proceeds with so much ardour as when he theorizes, and every effort to verify or to disprove particular speculations, necessarily leads to the evolution of new facts, and to the extension of the limits of useful knowledge. It is, therefore, the business of genuine philosophy rather to point out the imperfections, to expose the errors, and to restrain the presumptuousness of the theorist, than to attempt the entire extinction of a spirit which, however incomplete and insufficient the materials on which it has to work, must at least facilitate generalization, smooth the paths of knowledge, and render the approach to truth less rugged and tedious.

Having now attempted to bring before you a brief account of a few of the principal geological theories, with a view to give some insight into the objects of this branch of science, considered in the abstract, I trust I may recommend this pursuit, as embracing in its secondary applications, a variety of useful and popular information.

In mining, in the search for coal, in the structure of canals and roads, in building, draining, and in the judicious search for and management of springs, the advantages of practical geology are incalculable. It has frequently happened that materials for roads have been transported at great expense from distant parts, when they might have been abundantly procured in the neighbourhood; in sinking wells injudicious situations have often been pitched upon, and buildings erected at a distance from copious sources of water. In the fruitless search for metallic veins, millions have been expended upon the delusive promises of ignorant adventurers; and coal is frequently sought for in places, which even a slight knowledge of the subject indicates as hopeless.

The arrangement which I propose to adopt in the present course is too simple and obvious to need any detailed explanation. We shall commence with an examination of the superior or uppermost strata, and more particularly examine into that wonderful history of the devastation and destruction of a former order of things which is exhibited by the various alluvial and diluvial matters that cover the uppermost of the secondary strata.

These secondary strata we shall then examine in their natural succession—the chalk, with its contents and accompaniments will claim much of our notice; and here, and in the superincumbent beds, we shall find brick, earth, potters' clay, and various other products applicable to the arts; here, too, we shall discover the seat of those subterraneous rivers, which, flowing from more elevated situations, make their appearance at different depths, and in different strata, constituting the various springs of hard and soft water about the metropolis.

The next substances that occur, are the varieties of freestone used in rough sculpture and building, such as those of Bath and Portland, with their various argillaceous concomitants; and to these succeed the red sandstone and marl strata in which are our deposits of rock salt, and with which are associated the still more important formations of coal and iron-stone; the whole of these strata covering a vast extent of country, lie in enormous cavities or basins bounded by that variety of marble, commonly called mountain limestone, in which, in Derbyshire, and several of the northern counties, are also various subterranean treasures, and whence, more especially, are derived the enormous supplies of lead which enrich the British market. The truly metalliferous formations, or those rocks and strata in which the veins of the great mining districts of England occur, will now be exposed to our view; and we ultimately arrive at granite and its associates, which form the great and primæval mountain chains of the world, and upon which, as far as our limited inquiries enable us to ascertain, all the other rocks are incumbent.

Having thus examined the strata of our globe in the order of their succession, from the surface downwards, I propose to de-

vote some lectures to the structure, position, contents, and theories of metallic veins; to a general account of the relative durability of rocks and mountains; and to an examination of the causes which are tending to their disintegration and decay—connected with this subject is the nature and formation of soils, and some other topics, which may prove of general use and interest.

I shall conclude my course with some observations connected with the causes and effects of volcanic fires, with the phenomena of earthquakes and boiling fountains, and with some remarks relating to other limited phenomena, as far as they tend to elucidate or explain any mysteries and difficulties which we may have met with in our previous inquiries.

Hastily as we have now gone over the subjects of geological research, I shall consider my intention in this introductory discourse as amply attained, if I have convinced you that it is no unworthy or unimportant branch of science; but that, on the contrary, it embraces enticing, useful, dignified, and even sublime materials of discussion and instruction.

It surely concerns us all to know something of the ground we tread upon, of the country we inhabit, and of the sources, and natural association of the infinitely-varied products, with which the mineral world assuages our wants, increases our comforts, and multiplies the luxuries of life.

To the traveller, geology opens, as it were, a new creation; in connexion with geography, it discloses what may be termed the physiognomy of the earth; thus clothing a barren country with numerous objects of interest, and giving a new zest to those pleasing emotions of the mind with which we behold the fertile landscape and fruitful plain; and to those more exalted sensations which are created by the majestic features of a rocky and mountainous district.

Finally, in displaying to us, as it does, in characters most unequivocal, the great and awful revolutions which this earth has suffered, it gives rise to a salutary reminiscence of those which yet may come; it shows us upon what slight foundations the pre-

sent order of things rests, and how trivial a change would suffice to obliterate all that now exists; thus inspiring the well-attempered mind, not with sorry fancies and idle fears, nor with that superstitious awe which sometimes results from gross ignorance, and sometimes from perverted knowledge, but with deep and unshaken admiration of that boundless wisdom which governs the revolutions of nature—which,

> Builds life on death, on change duration founds,
> And bids the eternal wheels to know their rounds.

II.

Having noticed some of the leading subjects of geology, and briefly enumerated some of the hypotheses entertained by our precursors in this department of physico-chemical science, I shall proceed more particularly to explain its objects, as they are now pursued, and endeavour to sketch an outline of the study, as it exhibits itself at the present day.

Its first and leading object is to become practically acquainted with the present state of the Earth's external structure; for, excepting of its crust or rind, we know nothing; and all that has been suggested, either by theory or experiment, relating to its internal *composition*, its density, and the constitution of the entire mass, is mere surmise and guess-work—deductions hastily drawn from superficial observation, or unwarranted inferences from imperfect researches.

The present surface of our planet is composed of lapideous materials, the nature and composition of which, it is the business of mineralogy and of chemistry to determine; not that the minutiæ of either of those studies need, of necessity, be gone into by the geologist, for the substances which thus present themselves are, comparatively, few in number and simple in their nature, and their external characters and intimate composition are soon learned; yet, if he would pursue his subject under every advantage, and extend his inquiries into its more refined departments, he must neither be a superficial mineralogist, nor an imperfect

chemist. A knowledge of the crystalline forms, and general mechanical characters of substances must often be called to the aid of his speculations, and he can frame no plausible theory without combining with such information a just, and even minute, acquaintance with the effects of heat and various solvents upon mineral masses, and upon the parts of which they consist.

Without these guides, therefore, he who aims at any thing beyond mere practical geology, will infallibly go astray: they are lights essential to his successful progress, and the wanderings of those who would penetrate into the more secret parts of geology without them, are abundantly manifest to the intelligent reader, in most of those speculative writers, whose eloquence and dexterity in argument may mislead the unwary into an acquiescence in their reasonings, but which, when measured by less falliable standards, are found void of solidity and truth.

Siliceous, calcareous, and argillaceous substances, either pure or nearly so, and in a state of mixture, or loosely and indefinitely blended, rather than in strict chemical combination, constitute a very large relative proportion of those rocky masses, or scattered or comminuted substances, which form, or have formed, the most exterior constituents of our planet; and of these, considered in the abstract, the chemical and mineralogical history is soon told.

Under the name of *rock crystal*, or *quartz*, SILICA presents itself nearly pure; and in the varieties of flint, agate, calcedony, and siliceous sand, it is, by far, the predominating ingredient. Its most usual crystalline form is a six-sided prism, terminated by a six-sided pyramid; but its primitive form is an obtuse rhomboid, nearly approaching the cube. In specific gravity the pure varieties fluctuate a little on one side or other of 2.6. It cleaves with great difficulty; its common fracture is conchoidal. There are many varieties as to form and colour, which chemical mineralogists describe and distinguish; they are all hard enough to scratch glass. The leading *chemical* characters of silica are, extreme difficult fusibility; insolubility in water, and in nearly all acids, except under certain peculiar circumstances of recent

precipitation, under which it readily dissolves in potassa and soda; it is also easily combined with those alkalies by fusion, such compound constituting the basis of glass, and where the alkali greatly predominates, being soluble in water. This solution is called, sometimes, *liquor of flints;* the acids decompose it, and throw down a finely-divided *hydrate of silica*, which is soluble, to a small extent, in water; a fact of some importance in relation to our present subject, and accounting, in some measure, for the existence of silica in certain mineral waters, and more especially in those of the boiling fountains or geysers of Iceland, which deposit it in abundant incrustations. Calcedony, and some other minerals, are also occasionally referred to, as attesting the once fluid state of silica from a watery solvent: its crystalline forms may have originated from igneous fusion, but the existence of aquatic *confervæ*, often of their native colour, or discoloured by oxide of iron, and of mosses and lichens, in certain agates, renders fire, in many instances, an inadmissible agent. Upon these subjects, Dr. Mac Culloch's inquiries are extremely deserving notice. Even the chrysalis of a moth is, it is said, extant in agate. One other chemical character of silica must not be overlooked, as bearing upon geological theory, which is, its affinity for other earths, either in igneous fusion, or aqueous solution. In the former way, it combines with lime and alumina, with magnesia, baryta, and strontia. Pottery and porcelain are alumino-siliceous compounds, with an occasional proportion of magnesia. Three parts of silica, 2 of magnesia, and 1 of alumina fuse into a glass; and we shall, by and by, find several compound minerals of analogous constitution. Nor must the humid attractions of silica be overlooked; for when its alcaline solution is added to aqueous solutions of lime, baryta, or strontia, or to an alcaline solution of alumina, compound precipitates of the earths are the results, several of which have been particularly examined by Mr. Dalton. (System, p. 341.) It may be right to add, that silica is, probably, a binary compound of oxygen with a peculiar inflammable base, which has been called *silicon*, and that it appears to contain about half its weight of

oxygen; but no application of this subject to geology renders it necessary to dwell upon it, however interesting as a branch of chemical inquiry, and as shewing how great a proportion oxygen constitutes of the solid, as well as of the fluid, matter of our globe.

Carbonate of Lime is a very predominant ingredient in rocks —marble, chalk, oolite, freestone, and all the purer varieties of limestone, are essentially composed of it, and these substances form strata of prodigious extent. It predominates in all the varieties of shell, coral, and madrepore: with other substances, but especially silica and alumina, and a little oxide of iron, it is found in lias, and several marls and clays. Calcareous spar and statuary marble may be selected as this substance in a very pure state. The former, when transparent, is highly doubly refractive, as we see in Iceland spar. Its crystalline forms are very various, but they all result from a *primary rhomboid*, the angles of which are 105° 5′ and 74° 5′. Exposed under ordinary circumstances to heat, it loses carbonic acid, and leaves quick lime, and by such an experiment its composition is shewn to be 28 lime + 22 carbonic acid, or *per cent.* 56 and 44: but if it be exposed to heat and pressure, so as to prevent all escape of gaseous matter, it then fuses, and retains its carbonic acid; a fact of no small importance as connected with certain theoretic considerations of the Huttonian school of geology. Carbonate of lime is easily recognised by its softness, and by the effervescence which it produces when a little dilute muriatic acid is dropped upon it; and as none of the other carbonates constitute mineral masses, this criterion is alone sufficient to distinguish it under such and several other circumstances. Or, the presence of lime in solution may be detected by oxalate and by carbonate of ammonia, which, added to muriate of lime, occasion precipitates of oxalate and of carbonate of lime; both these yield quick-lime upon exposure to a sufficient red heat.

Alumina rarely occurs pure in nature, but it is very abundant in conjunction with other earths, and is present in all clays and *plastic* earthy compounds, on which it confers the property of

exhaling an earthy odour when breathed upon. It is nearly infusible when pure; but, as has already been stated, it enters into combination with the other earths, so as to produce fusible compounds. When in a very comminuted state, it dissolves in the caustic fixed alkalies, and in most of the acids;—with potassa and sulphuric acid, it forms the characteristic octoedral crystals of common alum. Alumina has so strong an attraction for water as to retain it for some time, even at a red heat. In its pure and crystalline form, it constitutes the gem called *sapphire*, of which there are several coloured varieties.

MAGNESIA, like lime, forms a part of several compound minerals, most of which have a greenish colour, and a soapy feel. Mixed in the state of carbonate, with carbonate of lime, it is found in a subspecies of limestone, to which it imparts peculiar characters. It is insoluble in the alkaline solutions, but readily soluble in many of the acids; and its presence is announced by a precipitate of carbonate of magnesia, on adding carbonated fixed alkali to its solution; while, on the other hand, bi-carbonate of ammonia, which throws down lime, does not precipitate magnesia unless phosphate of soda be added; and upon this is founded an elegant method of detecting the presence of magnesia, devised by Dr. Wollaston. Now the different sands, clays, marls, and limestones of the upper strata, are composed of mixtures of the substances just enumerated, and they constitute, in chemical combination, and with a few unimportant additions, the principal components of the primary rocks.

The regular succession of the Earth's strata has been already partially referred to, as determined by the inquiries of some of the earlier geologists; and as the order of this succession, and the respective characters of the series, will occupy much of our attention, it may be right to explain it by a few preliminary observations. Let us suppose a traveller, for instance, departing from London and travelling westward towards North Wales; and let us direct his attention to the ground which he passes over—he first traverses a tract of clay and sand, then he enters

upon chalk, which is succeeded by calcareous freestone and a species of argillaceous limestone; then comes a zone of red sandy marl, and mines of coal and iron succeed, surrounded by limestone, and followed by slate and granite.

If, instead of proceeding westward, his route lie to the north, the same succession of strata, the same rocks and mineral masses, exactly in the same general order, will present themselves, and various opportunities will occur to lead him to another important fact respecting these stratified beds, which is, that they are evidently disposed at an angle, and neither vertical nor parallel to the horizon; so that their edges may be observed successively to emerge from under each other. The term *outcrop* or *basset* of the strata is applied to the successive zones thus formed, and their inclination or dip is found subject to much variation.

It is impossible to contemplate this arrangement without discerning the important secondary purposes to which it so efficiently contributes; for those strata which at one place are at impenetrable depths, are at another so brought to the surface, as to enable us to examine and obtain them and their contents: numerous useful products and mineral treasures are thus collected at, or comparatively near, the surface, with which we otherwise must have remained wholly unacquainted, or which could only have been procured by almost insurmountable labour and expense. Without such obliquity of stratification, there would have been no succession of soils. "In the whole machinery of springs and rivers, and the apparatus that is kept in action for their duration through the instrumentality of a system of curiously constructed hills and valleys, receiving their supply *occasionally* from the rains of heaven, but dispensing them *perpetually* in thousands of never-failing fountains," we see other important consequences of this arrangement of rocks. Waters, collected upon the hills and on high ground, filter and flow through the softer and permeable layers, producing springs in the valleys, and feeding streams and rivers, instead of accumulating in marshes and swamps as they would do, were the strata horizontal, or the surface plane.

Among the strata or formations of the vicinity of London,

chalk forms a very conspicuous feature; but there are, lying upon it, a variety of clays and sands, and beds of gravel, which will claim our earliest notice. Beneath the chalk, another and a distinct series of sands and clays will be found to prevail, which are incumbent upon calcareous freestone and lias: to these succeed marls and sandstones deeply tinctured by red oxide of iron, and often containing detached masses, as it were, of rock salt and alabaster. These may be considered as constituting one comprehensive and important subdivision of the strata. The next includes the coal deposits, and the sandstone and limestone with which they are more immediately associated, or upon which they are incumbent. The third subdivision is characterized by the prevalence of slaty or schistose rocks; and the fourth is confined to granitic aggregates, of which there are many sub-species.

We thus find the coal strata interposed between two great families of rocks, which, with Messrs. Conybeare and Phillips, we may call the *supermedial* and *submedial* orders; coal and its associates are the *medial* order, and granites on the one hand, and alluvial and diluvial matters on the other, form the *inferior* and *superior* series. Using these terms, we lose sight of all theoretical distinctions—of primary, transition, and secondary formations.

The medial order of rocks, and all above them, are characterized by containing remains of vegetable and animal tribes; and these are sometimes in great profusion; but they are far from being indiscriminately scattered through the strata; on the contrary, " they are disposed as it were in families, each formation containing an association of species, peculiar in many instances to itself, widely differing from those of other formations, and accompanying it throughout its whole course; so that at two distinct points on the line of the same formation, we are sure of meeting the same general assemblage of fossil remains."

In the carboniferous limestone, for instance, forming, as we have said, a member of the medial series, we always find the same corals, encrinites, terebratulæ, &c., from whatever part of the world our specimen comes. In the chalk, too, there is an asso-

ciation of shells, echini, and so on, peculiar to it; but if we compare the relics in the mountain limestone with those of the chalk, we shall find that they, in all cases, are perfectly distinct; that there is not, even in any single instance, a remote resemblance or analogy between them. Even between contiguous beds, there are often, in this respect, very striking distinctions, and the whole subject is of so singular and problematical a nature, as to have attracted, in an especial manner, the attention of the geological theorist. We have, for instance, as the lowest crust of the globe, or as its *nucleus*, as some mechanical philosophers will have it, a compound crystalline rock, generally very hard and permanent, in which the most inquisitive eye has as yet traced no organic remains of either kingdom of nature, no rounded pebbles, or any relic or detritus of other rocks. Symptoms of disturbance, perhaps of fusion, it does, indeed, exhibit, as our specimens will hereafter teach us. On this granitic foundation is reared a slaty structure; and in the rocks between it and the coal formations, animal remains, characteristically different from any which now exist, begin to make their appearance, and become abundant in the carboniferous limestone. Rounded pebbles, and other proofs of the existence and destruction of former rocks, are also here met with; but, what is not a little remarkable, we find in the coal strata themselves, which thus repose on the coral limestone, scarcely a single shell or relic of the kind: but on the other hand, abundance of vegetable remains, all, I believe, of unknown species, but of genera often allied to plants that now inhabit tropical climes. In the magnesian limestone, resting upon the coal measures, marine remains are again frequent; but in the red marl, which next follows, scarcely a shell or plant of any kind occurs. In formations that intervene between the red marl and the superficial strata, that is, in the varieties of lias and freestone, in the ferruginous and chloritic sands, and in the chalk itself, we find peculiar corals and echini, the remains of testaceous and crustaceous animals, of marine oviparous quadrupeds, and of vertebral fishes, all of extinct species, but quite distinct from those which we found below the red marl, and often remarkably distinguished among

themselves according to the individual strata which they occupy. Lastly, in the uppermost and superficial strata, we find shells, not, as in former cases, changed, petrified, or lapideous, but in such a state, that when washed and cleaned, they might pass for recent; some of these correspond to marine shells, and others, in other strata or beds, resemble our present fresh-water shells; whence the alternate inundation on certain spots of fresh and salt water has been inferred. In the most superficial and highest of these strata, we at last arrive at shells such as now exist, and then comes that great tell-tale of the deluge, the gravel, in which we discover the remains of land quadrupeds of unknown genera, and of extinct species, and, commonly, the latter correspond to inhabitants of very different climates at present.

Another important circumstance upon which I have only slightly touched, is the very frequent occurrence of rounded pebbles composed of fragments of the older rocks, which are abundant in the secondary strata. These pebbles prove the antecedent formation and consolidation of the rocks of which they are the debris; and they also shew another curious fact in the history of stratification, which is, that where they occur, as they often do, in vertical beds, they must have acquired that verticality by the operation of some force, which has thrown them out of their original horizontal position; for we cannot suppose it possible that any extensive accumulation of loose gravel can have taken place upon surfaces greatly inclined to the horizon.

Such beds of consolidated gravel are common in the red sandstone below the carboniferous series, in that series in the lower strata of the superior red sandstone, in the sand below the chalk, and in the gravel immediately above it and below the London clay.

Now it is impossible to contemplate the collection of marine relics which are so abundant in many of the strata, and which are found upon the summits of some of the loftiest mountains of the world, without immediately coming to the conclusion that our present land has not been merely transitorily covered by the waters of an inhabited ocean, but that it has actually been

formed of materials collected through a series of ages in the bosom of an aqueous abyss; and one of the first problems we are called upon to solve, is the cause of the change of level in this ocean, and of the emersion of our present land. It is difficult to conceive any considerable change in the actual quantity of water, resulting either from its transmutation or decomposition; yet such causes must not be excluded as impossible. But the most obvious solution of the difficulty is founded upon the supposition that certain powerful agents have elevated our present continents, and at the same time depressed the bed of the ocean; what was once, therefore, the bottom of an antediluvian sea, now appears to be our habitable land; and perhaps the dry land of a very remote period of the world, may be the bottom of the present sea. This disruption and elevation of the strata is not by any means a mere gratuitous assumption, for independent of the verticality of loose gravel beds, and other highly inclined strata which must have been once vertical, we have so many other instances of dislocations and heavings up of the strata among the old as well as among the newer rocks, as, in the opinion of some, to demonstrate to the utmost certainty the agency of an irresistible elevating force; but here the inquirer will perhaps stop to ask *what force* or *what power* in nature could have been adequate to such extraordinary and gigantic effects; a question which must be answered by minute inquiry into the position of the strata, their associations, texture, and derangements, and by a comparison of these with powers and causes now active or subject to our control and investigation.

Another geological consideration, independent in a manner of the former, but of much interest, and of some difficulty, relates to the causes which have been active in producing the present *more superficial appearances* of the earth; in carving it out into valleys, and fitting it as it were for the order of things which now prevails.

Valleys are more or less extensive furrows of the surface, ramifying generally to a considerable lateral extent, and independent of secondary purposes, fulfilling that most essential one of drain-

ing the adjacent lands of their water, and carrying it in brooks and streamlets, which gradually unite to form rivers, and ultimately convey their contents into the ocean, of which, in fact, they constitute a series of ascending branches.

Now there are some valleys and river channels which appear to owe their origin to some great convulsion or catastrophe, which has torn asunder the rocks and strata, and left certain intervening chasms; but in by far the greater number of instances we have no evidence whatever of the activity of such agents, and every thing indicates a less sudden and violent, but yet an energetic, cause.

Valleys frequently intersect the strata which they traverse, in such a way as to leave no doubt of their subsequent formation to that of their bounding rocks, a phenomenon almost always observable, and sometimes within very narrow limits; as where in a mountainous country a rapid river cuts through a narrow defile; on these occasions we observe the same strata repeated upon each wall, as it were, of the valley, the soil or substratum of which consists of the lowest layer of the series. Now when we witness such appearances, and when among the pebbles, and sand, and fragments of the low-land we find comminuted portions of the surrounding rocks, when we discover a collection of such debris in the soil and bed of the river, we almost necessarily arrive at two conclusions; one, that the strata once were continuous; and the other, that the intersecting agent has been water: water, not flowing as it now does, quietly through the valleys, but constituting mighty and destructive torrents, which, while they have intersected, have at the same time, in many instances, denuded the strata; which sometimes working upon soft or muddy materials, have transported them to a distance, but which have also ground down the hardest substances, and aided by the gravel and debris thus formed and impetuously hurried along, have chiselled out furrows in the more indurated and primary rocks. Some have vainly imagined the present streams as adequate to the production of such effects; but he whose sight is not dimmed by hypothetical blindness, will see evidences of a more powerful agent, and will at

once, and plausibly fix upon the deluge. I leave the proofs of such irresistible and extended currents to future lectures; but of their general agency, the instance of intersection and denudation to which I have alluded affords strong confirmation; for it sometimes happens that in insulated hills, in *outliers*, as geologists sometimes call them, we discover, although at a considerable distance from the chain with which they were once connected, some of the lower ranges of those strata, the upper ones having been washed away.

But although we find it necessary to refer, for satisfactory explanation of much that we now see, to that period when the earth "was covered with the deep as with a garment," and when "the waters stood above the mountains," it is by no means intended to exclude altogether the influence of later and partial inundations, of which we shall duly take notice, but which are manifestly inadequate agents to the production of the effects just mentioned. Hence the necessity of a distinction of the terms applicable to them, and the propriety of designating the water-worn fragments, and pebbles, and debris of all the rocks, by the term *diluvial* products, while we limit the term *alluvium*, to the sandy and muddy collections of our present currents: these are seldom carried far; they form flats and deltas near the mouths of rivers, and accumulations of finely-divided matters in their beds; but of the comparatively gigantic force of diluvial currents ample testimonies are extant; and when we find pebbles which have obviously been thus transported for many miles, and even huge blocks which have suffered a corresponding transplantation and attrition, and lodged in valleys separated by lofty ranges of hills from the rocks whence they must have had their origin, we may not only form some idea of the force of the torrent, and the power of attrition of the substances impetuously hurried along by it, but we also read in such phænomena the most unequivocal evidence of the non-existence of many of the deepest valleys and most striking irregularities of surface which now exist, at the time that the boulders and pebbles were transported.

Several geological hypotheses, and the Huttonian theory in particular, affect to descry, in the agency of existing causes, the

source of those effects which we have referred to extraordinary and occasionally acting forces; they have assumed the present rivers as the excavators of their own valleys, and ordinary volcanic fires as the indurators and elevators of strata from the bosom of the deep; they think that the washing down of finely-divided matter formed by the action of air and water upon the present surface, and the inroads of the ocean as manifested by the abrupt precipices and shingles of the beach, are sufficient evidence of the slow but sure destruction of the present order of things; but such actions are not only of very circumscribed extent, they are absolutely inefficient; and even if we draw unlimitedly upon *time*, as the theorists, in opposition to all chronological evidence, have done, their tendency is often the very reverse of that which they would substantiate. The fact is, that the wasting and wearing causes that now exist, are either too trifling to be taken into the account, or are counterbalanced by renovations and re-accumulations; in proof of this the barrows of the aboriginal Britons have been well adduced by Mr. Conybeare *, as retaining the pristine sharpness of their outline, after a lapse of little less, and probably more, than two decades of centuries: even the fosse that surrounds them is not filled up. "Causes, then, which in 2000 years have not, in any perceptible manner, affected these small tumuli, often scattered in very exposed situations upon the crests of our hills, can have exerted no very great influence on the mass of those hills in any assignable portion of time which even the imagination of a theorist can allow itself to conceive; and where circumstances are favourable to a greater degree of waste, still there is often a tendency to approach a maximum at which further waste will be checked; the abrupt cliff will at last become a gentle slope, and that slope become defended by its grassy coat of proof." Even the ocean itself throws up shingle banks and marsh lands, which check its further inroads. So that these supposed destructive powers, as they seem to the superficial observer, or to the biassed theorist, are soon found to

* Conybeare and Phillips: Outlines of Geology, page xxxii.

neutralise themselves, and are certainly inadequate as general agents, though their occasional and local effects may, if not duly weighed and compared, mislead us in the estimate of their powers. Indeed, of accumulation rather than of decay, of growth rather than of wasting away, we have further remarkable illustration in the phænomena of submarine and subterranean forests of trees manifestly buried in their natural position, as on the coast of Lincolnshire, Pembrokeshire, and Lancashire, in the valley of the Thames near Purfleet, and elsewhere. We also have instances of the agglutination of sand into sandstone, on the north coast of Cornwall; of æstuaries filled up by alluvial debris, as in the Cornish stream-works; and many other attestations of the extension rather than the destruction of habitable surfaces.

Having now sketched the business of the succeeding lectures, and having briefly enumerated the various theories which it will be my object more fully to discuss and explain, I shall proceed to examine, in detail, the most superficial strata of the globe, and to pursue that plan of geological inquiry of which the heads have been enumerated.

(To be continued.)

Art. IX.—*On the Hygrometric Properties of Insoluble and Difficutly-soluble Compounds. By* Mr. T. Griffiths.

[Communicated by the Author.]

The power of absorbing moisture from the atmosphere has been shewn by Professor Leslie not to be peculiar to acids, alkalies, and deliquescent salts, but to be also possessed by several insoluble chemical compounds.

His experiments on these are not, however, very numerous, and are all chiefly in reference to the degree of dryness they indicate upon the hygrometer, when submitted to the action of air saturated with aqueous vapour. The subject appearing one of con-

siderable importance, and likely to offer many new and useful results, a more extended series of experiments were undertaken, with the view of determining the actual increase, sustained by given weights of various insoluble bodies, on exposure to a very damp atmosphere.

Portions of the various oxides, chlorides, and salts of earths and metals, were reduced to fine powder, and dried on a sand bath, till they ceased to deposit moisture upon a cold glass plate, held over their surfaces; 100 grains of each were then accurately weighed, and placed in small glazed earthenware pans, about three inches in diameter, and a quarter of an inch deep; these were all respectively numbered, and a list of their contents provided. The substances being thus far arranged, were placed in parallel rows at the bottom of a shallow box, which was now taken into a small out-building, about six feet square, the atmosphere of which was saturated with moisture. This was completely effected by covering one of its sides with flannel, kept constantly wetted from a vessel of water placed near the roof of the building.

The cover of the box was slightly raised, so as to allow free access of air, and at the same time exclude any particles of dirt or dust that might otherwise have fallen on its contents.

Upon making an experiment with an hygrometer in this atmosphere, the temperature (45°) closely coincided with the dew point; thus proving its saturation with aqueous vapour. Under these circumstances the various substances remained for a month, during the whole of which time the atmosphere was kept saturated; at the expiration of this period the temperature of the air and dew point being 37°, they were again all accurately balanced, and their increase of weight carefully noted. These results, together with the names of the substances employed, are given in the first of the annexed Tables.

Spongiform silver, powdered bismuth and antimony, of each 50 grains, dried, and exposed with the above substances, acquired no increase in weight.* A similar quantity of spongy platinum gained however .1 of a grain.

It is well known that newly-made charcoal has the property of

absorbing air and moisture with such avidity, that it can hardly be removed from one vessel to another, without increasing in weight. This takes place in charcoal from different woods, with various degrees of force; the second Table shews the increase in every hundred parts of charcoal from thirteen different woods, weighed whilst very hot, and exposed for a week to the above atmosphere.

Oxide of zinc	29.
——— chrome	10.
——— iron (colcothar)	3.1
——— manganese (black)	2.5
——— lead (litharge)	1.7
——— bismuth	.7
——— iron (scales)	.5
——— lead (red)	.2
——— mercury, by nitric acid	.2
——— copper (black)	.1
——— tin (putty)	.1
Chloride of silver	.6
——— lead	.5
Sub. mur. of copper	1.8
Chromate of lead	.5
——— mercury	.1
Sulphate of lime	16.2
——— lead	.4
——— baryta	.3
——— strontia	.1
Sulphuret of antimony (liver)	.9
——— antimony (black)	.4
——— mercury (cinnabar)	.4
Bi-sulphuret iron	.2
Phosphate of lead	.5
Tartrate of lead	.7
Plumbago	4.5
Carbonate of lead	.6
——— zinc (calamine)	.5

Carbonate of lime (chalk)	.3
——— baryta (native)	.2
——— strontia (native)	.1
Aurum musivum	.2
Clay iron stone	.5
Smalt	2.1
Fluor spar. Blue variety	.4
Cornish clay	2.4
Serpentine	5.2
Mica slate	1.1
Drawing slate	1.
Zeolite	.3
Granite	.2
Silica—powdered quartz	.2

Charcoal from walnut-tree	17.3
——— tulip-wood	15.4
——— ash	15.3
——— Botany-bay wood	15.2
——— launce-wood	13.7
——— cedar	13.4
——— America pine	12.6
——— willow	12.1
——— birch	12.
——— rose-wood	12.
——— lime-tree	11.8
——— king-wood	11.5
——— Zebra wood	6.6

Foolscap paper	18.2
Cartridge	17.1
Brown	15.3
India	11.6
Filtering paper	5.

Known weights of these various specimens of paper, dried strongly before a fire, were exposed to the atmosphere for 24 hours, and gained the annexed increase for every 100 parts.

The respective quantities of moisture, absorbed by all the substances, may very probably vary according to the method of their preparation and state of mechanical division, but as far as general results are concerned, they will not differ very widely from the above.

Art. X.—*On the Production and Nature of Oil of Wine, (Oleum Æthereum of the London Pharmacopœia,) by* Mr. H. Hennell, Chemical Operator at Apothecaries' Hall.

[Communicated by the Author.]

Mr. R. Phillips, in his translation of the London Pharmacopœia, appearing to doubt the existence of Oil of Wine, as a distinct substance, I was induced carefully to repeat the process we usually adopt in our laboratories for obtaining it.

Half a gallon of rectified spirit of wine (Sp. Gr. 830,) was mixed with an equal bulk of sulphuric acid, and distilled in a glass retort; the products were ether, water, sulphurous acid, and about four ounces of a yellow fluid floating upon the water, which, when separated and washed with solution of carbonate of potash as long as there was any trace of sulphurous acid, was a solution of true oil of wine in ether. The ether may be removed, either by spontaneous evaporation, or it may be distilled off with a very gentle heat. The oil thus obtained, and which amounts to about two ounces, is a yellow fluid, resembling, in appearance, oil of lavender or peppermint; perhaps rather more viscid. It has a specific gravity of 1.05. After being kept a few months, it becomes more viscid, and a number of prismatic crystals form in it, which, in many of their characters, very much

resemble Napthaline; they are soluble in ether and alcohol, and crystallize from both those solvents in very slender prisms; they melt with a very slight heat, and sublime unaltered; in warm sulphuric acid they dissolve, forming a pink solution; they dissolve in cold nitric acid, forming a deep red solution, similar to that of morphia in nitric acid; heat destroys this colour instantly, and the solution, after boiling, on being diluted with water, throws down a white flaky precipitate. The crystals are insoluble in muriatic and in acetic acids, and in the caustic alkalies, hot or cold.

The oil is soluble in ether and alcohol, but insoluble in water; distilled with water, it passes over like the greater number of the essential oils, without having undergone any alteration; but when a portion was attempted to be distilled alone, the greater part came over in the form of a thick oily matter, a considerable quantity of sulphurous acid was formed, and charcoal and a little sulphuric acid were left in the retort. With a view to get rid of a portion of acid, which the carbonate of potash had apparently not removed, some of the oil was heated in a solution of caustic potash; it diminished considerably in bulk, and became much more viscid than before: it was separated from the potash solution by the action of ether, and when the ether was distilled off, there remained a yellow oil, with very little fluidity, which evaporated entirely when heated, without any appearance of decomposition or evolution of sulphurous acid, and which, in a few days, concreted into a mass of prismatic crystals, having all the characters of those before described. The potash solution evaporated to dryness, afforded a residue somewhat like acetate of potassa in appearance; upon heating a few grains of it, it took fire, and burnt with a flame resembling that of alcohol, and sulphate of potash remained; it dissolved in hot alcohol, and the solution deposited, on cooling, crystals in the form of pearly scales; in short, it had those characteristics which have been ascribed to sulphovinate of potassa; I therefore consider *oil of wine* as a compound of sulphovinic acid, and the peculiar crystallizable oil which I have described.

There are two facts which render it probable that oil of wine, when obtained, as in the above process, from alcohol and sulphuric acid, is a product of the decomposition of sulphovinic acid; namely—first, that when alcohol and sulphuric acid are mixed in equal bulks, sulphovinic acid is formed in great abundance; nearly five ounces of sulphate of lead were obtained from the sulphovinate of that metal, formed by neutralizing the acid resulting from a mixture of four ounces of alcohol with an equal bulk of sulphuric acid, the mixture having been allowed to become cold before it was saturated—and secondly, oil of wine, or a fluid exactly resembling it, is obtained when any of the sulphovinates are carefully decomposed by heat.

Apothecaries' Hall, March 15, 1825.

Art. XI.—*Proceedings of the Royal Society of London.*

Thursday, Dec. 16, 1824.—At this meeting, Sir Charles Wetherell, Knt., and John Bell, Esq. were elected Fellows of the Society, and a paper was communicated by Dr. Roget, entitled an *Explanation of an Optical Deception in the appearance in the spokes of a wheel, seen through vertical apertures.*

A paper was also read containing a description of a *New Photometer*, by Mr. Ritchie.

Thursday, Dec. 23.—Captain F. W. Beechy was elected a Fellow of the Society.

Two supplementary papers to Mr. Powell's former communication, upon *Radiant Heat,* were read, and the Society then adjourned over the Christmas Vacation.

Thursday, January 13, 1825.—Mr. William Scoresby was admitted into the Society.

A paper was communicated by Captain Henry Kater, being a description of a *Floating Collimator.*

January 20.—Captain James Mangles, R. N., was elected a Fellow of the Society.

A paper on *the Construction of the Barometer* was communicated by John Frederic Daniell, Esq.

January 27.—The Rev. George Fisher was elected a Fellow of the Society.

Part of a paper on *the Anatomy of the Mole Cricket*, by Dr. John Kidd, was read.

February 3.—Viscount Strangford was elected a Fellow of he Society, and the reading of Dr. Kidd's paper was resumed and concluded.

Sir Everard Home gave in a postscript to his Croonian Lecture, in which he announced the discovery of nerves in the human placenta and umbilical chord.

February 10.—A paper on the *Iguanodon*, by Gideon Mantell, Esq. was read.

February 17.—Mr. Henry Harvey was elected a Fellow of the Society.

A paper was communicated by the Rev. Baden Powell, entitled *An Experimental Inquiry into the radiant heating effects from Terrestrial Sources.*

February 24.—Dr. John Richardson and Joseph Henry Green, Esq. were elected Fellows of the Society,

Part of a paper on the *Maternal Fœtal Circulation*, by Dr. Williams, was read.

March 3.—Dr. Lewis Tiarks was elected a Fellow of the Society, and the reading of Dr. Williams's paper was resumed and concluded.

A paper was read, containing *Further Observations on the Planariæ*, by Dr. I. R. Johnson.

March 10.—Major-General Sir William D'Urban was elected a Fellow of the Society.

A paper by Mr. W. Ritchie was read, entitled *Improvements on Leslie's Photometer.*

March 17.—A paper was communicated by Sir Everard Home, on *the Influence of Nerves and Ganglions in producing Animal Heat.*

Art. XII. ANALYSIS OF SCIENTIFIC BOOKS.

I. *Philosophical Transactions of the Royal Society of London; for the year* 1824. Part II.

The first paper in this part of the Transactions is entitled,

Some curious Facts respecting the Walrus and Seal, discovered by the examination of Specimens brought to England by the different ships lately returned from the Polar Circle. By Sir Everard Home, Bart. V.P.R.S. *In a letter addressed to* Sir Humphry Davy, Bart. P.R.S.

The first part of these "curious facts" is a peculiarity in the hind foot of the Walrus, which enables it to carry on progressive motion "against gravity;" this is effected by an apparatus resembling that of the foot of the fly, and in operation, not unlike a cupping glass: the anatomical details are illustrated by two plates; they exhibit, as far as bony structure is concerned, a striking resemblance to the human hand.

The second of Sir Everard's discoveries, is the mode in which the bile in the Walrus is collected in a reservoir, and thence impelled with great force into the duodenum. The anatomical structure, as shown in the plate annexed to this paper, is very peculiar.

Sir Everard's third new fact, is the peculiar structure of the funis and placenta of the seal; the trunks forming the funis are not twisted together; "their whole length is nine inches; three inches from the placenta they begin to give off branches, which freely anastomose with one another; these branches are connected to the placenta itself by three membranous folds, like so many mesenteries; between these folds the blood-vessels are conveyed to the substance of the placenta, on the surface of which they ramify to a great degree of minuteness. This structure will give a greater facility than common to the circulation through the placenta, which makes it an object of inquiry, whether the same peculiarities exist in other marine animals."

In another communication, printed in this part of the Transactions, Sir Everard describes the organs of generation of the Mexican Proteus, but the details of this paper require the plates to render them intelligible; they are engraved from Mr. Bauer's exquisite drawings, and display some singular physiological provisions.

Additional Experiments, and Observations, on the application of Electrical Combinations to the preservation of the Copper Sheathing of Ships, and to other purposes. By Sir H. Davy, Bart. P.R.S.

An abstract of this paper will be found at page 279 of our seventeenth volume.

On the Apparent Direction of Eyes in a Portrait. By W. H. Wollaston, M.D., V.P.R.S.

Of this paper we have also already given some account. (Vol. XVII. p. 274.) It is accompanied by several well-devised engravings, which might, we think, furnish the basis of a series of very amusing and instructive illustrations of the rules of perspective.

Further Particulars of a Case of Pneumato-thorax. By J. Davy, M.D., F.R.S.

The details of this paper will be found in one of our preceding numbers *.

On the Action of finely-divided Platinum on Gaseous Mixtures, and its Application to their Analysis. By William Henry, M.D., F.R.S.

The curious discovery of Professor Doebereiner, of Jena, respecting the ignition of platinum, when in a spongy form, on the contact of a mixture of oxygen and hydrogen, is well known to our chemical readers; and we have detailed, from the *Annales de Chimie*, several interesting facts respecting it, ascertained by MM. Dulong and Thenard. To these Dr. Henry has made some important additions.

The first section of his paper relates to the action of finely-divided platinum on gaseous mixtures, at common temperatures, and the second to its effect at increased temperatures; of these results an abstract has already been given in a former number of this Journal †.

* No. XXXIV. page 263. † No. XXXIV. page 277.

In the third section, he applies the facts detailed in the former two, to the analysis of mixtures of combustible gases in unknown proportions. For this purpose, he caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period, and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because, from former experience, it was expected to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas, the production of which is confined to the early stages of the progress. After washing it, therefore, with liquid potash, to remove a little carbonic acid, and ascertaining its specific gravity when thus washed to be .308, he proceeded at once to subject it to the following method of analysis.

Having ascertained, by a previous experiment with Volta's eudiometer, that ten volumes of the gas required for saturation 9 volumes of oxygen, he mixed 43 measures with 43 of oxygen (= 41 pure) and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat, and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43.5 volumes. Of these, 4.5 were absorbed by liquid potash, indicating 4.5 carbonic acid, equivalent to 4.5 carbonic oxide; the rest, being fired in a Volta's eudiometer with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also, circumstances which prove that 11 volumes of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed, (viz. 86 — 43.5=42.5) 2.25 are due to the carbonic acid formed; and deducting this from 42.5, we have 40.25, which are due to the oxygen and hydrogen converted into water; and $40.25 \times \frac{2}{3} =$ 26.8 shows the hydrogen in the original gas. But the sum of these numbers (26.8 + 4.5 + 11) being less by 0.7 than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows:

Hydrogen	26.8	62.32
Carbonic oxide	4.5	10.50
Carburetted hydrogen	11.0	25.56
Nitrogen	0.7	1.62
	43.0	100. 0

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be .303*, which

* In this estimate, the specific gravity of hydrogen is taken at .0694; that of carbonic oxide at .6722; of carburetted hydrogen at .5555; and of nitrogen at .9728.

which coincides, as nearly as can be expected, with the actual specific gravity of the gas submitted to analysis, viz. .808. To place the correctness of the results beyond question, our author mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas, when he had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Notwithstanding this successful result, which was twice obtained, Dr. Henry observes that he should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine, before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious, from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between 0.3 and 0.6 of an inch; or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter varying with the quantity of gas to be submitted to experiment, which may be from half a cubic inch to a cubic inch or more. These, when in use, may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube, and the other for the stem of a thermometer, the degrees of which are best engraved on the glass.

"By means of these improved modes of analysis," says our author, "I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them, till I have had an opportunity of pursuing the inquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr. Dalton, in oil gas, and coal gas, which agrees with olefiant gas in being condensible by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen."

We have anxiously looked for Dr. Henry's further remarks upon the very interesting subject of this communication, more especially as relating to the constitution of oil gas, connected with which there are so many curious and anomalous circumstances, that we are convinced their thorough investigation would

throw much new light upon the nature of the compounds of carbon and hydrogen, respecting which very much remains yet to be ascertained.

A Comparison of Barometrical Measurement with the Trigonometrical Determination of a Height at Spitzbergen, by Captain Edward Sabine, F.R.S.

Of this paper we have already given an abstract,* and also of the following one, containing *Experimental Inquiries relative to the distribution and changes of the Magnetic Intensity in ships of war, by* George Harvey, Esq.†

Experiments on the Elasticity and Strength of hard and soft Steel, by Mr. Thomas Tredgold.

If a piece of very hard steel be softened, it is natural to suppose that the operation will produce a corresponding change in the elastic power, and that the same load would produce a greater flexure in the soft state than in the hard one, when all other circumstances were the same. Mr. Coulomb inferred from some comparative experiments on small specimens, that the state of temper does not alter the elastic force of steel; and Dr. Young's Experiments on Vibration led to the same conclusion (Nat. Philos. II. 403). But the subject appeared to require further investigation, and particularly because it afforded an opportunity of ascertaining some other facts respecting steel, which had not been before examined.

In making the experiments described in this paper, each bar was supported at its ends by two blocks of cast iron. These blocks rested upon a strong wooden frame. The scale to contain the weights was suspended from the middle of the length of the bar, by a cylindrical steel pin of about three-eighths of an inch in diameter. And as in experiments of this kind it is desirable to have the means of raising the weight from the bar, without altering its position, in order to know when the load is sufficient to produce a permanent change of structure, a powerful screw with a fine thread was fixed over the centre of the apparatus, by which the scale could be raised or lowered, when the cords on which the screw acted were looped on to the cross pin by which the scale was suspended.

To measure the flexure, a quadrantal piece of mahogany was fixed to the wooden frame; two guides were fixed on one edge of the mahogany, in which a vertical bar slided, and gave motion to an index. The bar and index were so balanced, that one

* Vol. XVII. p. 268. † Ibid. p. 261.

end of the bar bore with a constant pressure on the specimen, and the graduated arc over which the index moved was divided into inches, tenths, and hundredths; and thousands were measured by a vernier scale on the end of the index. By a screw at the lower end of the vertical bar, the index was set to zero, when necessary.

The first trials were made with a bar of blistered steel of a very good quality. It was drawn out by the hammer to a proper width and thickness, and then filed true and regular. It was then hardened, and tempered to the same degree of hardness as common files.

The total length of the bar was 14 inches; the distance between the supports [illegible] inches; the breadth of the bar 0.95 inches, and the depth 0.875 inches: the thermometer varied from 55° to 57° at the times of trial.

With a load of 54 lbs. the depression in the middle was		0.02 in.
	82 - - - - - - -	0.03 ,,
	110 - - - - - - -	0.04 ,,

The last load remained on the bar some hours, but produced no permanent alteration of form.

The temper of the bar was then lowered to a rather deep straw yellow, and it was tried again; when the same loads produced exactly the same flexures as before.

The temper was then lowered till the colour was an uniform blue, or spring temper; and the trials were repeated with the same loads; but the flexures were still the same.

It was now heated to redness and very slowly cooled. In this state the same loads still produced the same flexures; and the load of 110 lbs. caused no permanent change of form.

The bar was hardened again, and made very hard; in this state the same loads produced the same flexures; and

With a load of 300 lbs. the depression in the middle was		0.115 in.
	350 - - - - - - -	0.130 ,,
	580 - - - - - - -	broke.

When the bar was relieved from the load of 350 lbs. it retained a permanent flexure of 0.005 inches, which increased to 0.01 with the addition of 10 lbs. to the load.

Finding that a bar of much greater length might be tempered without difficulty, Mr. Tredgold had another bar made of the same kind of steel; the length of which being 25 inches, about double the flexure could be given with the same strain upon the material; and therefore any small degree of difference in the elastic force might be more easily detected, for the preceding experiments showed that if there be any difference, it must be extremely small.

The breadth of this bar was 0.92 inches; the depth 0.36

inches; and the distance between the supports 24 inches. It was soft, so as to yield easily to the file.

With a load of 18.6 lbs. the depression in the middle was 0.05 in.

37.0	- - - - - - -	0.10 „
47.0	- - - - - - -	0.127 „

The bar was then hardened, so that a file made no impression on any part of it, and the same loads did not produce flexures that were sensibly different from those in the soft state.

The temper was then lowered till it assumed an uniform straw colour; when with a load of

47lbs.	the depression in the middle was	0.127	inches.
85	. . . .	0.230	„
130	. . . .	0.350	„
150	. . . .	0.400	„

The load of 150lbs. produced a permanent set of 0.012, but 130lbs. produced no sensible effect. The loading was continued, and with

185lbs.	the depression in the middle was	0.50	inches,
385	. . . .	1.04	„

When 385lbs. had been upon the bar about a minute, it emitted a faint creaking sound, and consequently no more weight was added; in about fourteen minutes the bar broke, exactly in the middle of the length.

On comparing the fractures of the specimens, there was no apparent difference except in colour. The grain was fine, and equal; the small sparkles of metallic lustre abundant, and equally diffused; but in the harder specimen they had a whiter ground.

From these experiments it appears that the elastic force of steel is sensibly the same in all states of temper.

The height of the modulus of elasticity, calculated by the formula given by Dr. Young in his *Nat. Phil.* (Vol. II. p. 46,) is, according to the first experiment, ... 8,827,300 feet.
And according to the second experiment . . 8,810,000 feet.

Now the height of the modulus, as has been determined by Dr. Young for steel by experiments on vibration, is 8,530,000 feet. (*Nat. Phil.* II. p. 86.) The modulus for cast steel calculated from Duleau's experiments (*Essai Théorique et Expérimental sur le Fer Forgé*, p. 38,) is 9,400,000 feet, and for German steel 6,600,000 feet.

The force which produces permanent alteration is, to that which causes fracture in hard steel, as 350 : 580; or as 1 : 1.66; in the same steel of a straw-yellow temper as 150 : 385, or as 1 : 2.56.

When the tension of the superficial particles at the strain which causes permanent alteration, is calculated by the formula given in Mr. Tredgold's *Essay on the Strength of Iron*, p. 146, Second Edition, it is 45,000lbs. upon a square inch in tempered steel; and the absolute cohesion 115,000lbs. Mr. Rennie found the direct

cohesion of blistered steel to be 133,000lbs. (*Philosophical Transactions* for 1818.)

But in the very hard bar, the strain which produced permanent alteration was 51,000lbs. for a square inch, and the absolute cohesion only 85,000lbs.

From these comparisons I think it will appear, that in the hardening of steel, the particles are put in a state of tension among themselves, which lessens their power to resist extraneous force. The amount of this tension should be equal to the difference between the absolute cohesion in different states. Taking Mr. Rennie's experiment as the measure of cohesion in the soft state, it will be 133,000 — 115,000 = 18,000lbs. for the tension with a straw-yellow temper; and 133,000 — 85,000 = 48,000lbs. for the tension in hard steel. And if this view of the subject be correct, the phenomena of hardening may be explained in this manner, which nearly agrees with what Dr. Young has observed in his Lecture I, p. 644: after a piece of steel has been raised to a proper temperature, a cooling fluid is applied capable of abstracting heat more rapidly from the surface than it can be supplied from the internal parts of the steel. Whence the contraction of the superficial parts round the central ones which are expanded by heat; and the contraction of the central parts in cooling, while they are extended into a larger space than they require at a lower temperature, produces that uniform state of tension, which diminishes so much the cohesive force in hard steel. The increase of bulk by hardening agrees with this explanation; and it leads one to expect, that any other metal might be hardened if we could find a means of abstracting heat with greater velocity than its conducting power.

A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth. In a Letter to Thomas Young, M.D., F.S.R.S., and Secretary to the Board of Longitude. *By* Dr. John Lewis Tiarks.

The results of these observations are given at p. 270, Vol. XVII.

Of the Effects of the Density of Air on the Rates of Chronometers, by George Harvey, Esq.

We have elsewhere shortly noticed the contents of this paper*, which occupies forty of the quarto pages of the Philosophical Transactions; we must therefore refer those who are interested in the minute details of such an inquiry to the original document.

* Vol. XVII. p. 272.

A Letter from L. W. Dillwyn, Esq. to Sir H. Davy, Bart., P.R.S.
(See an abstract of this Letter, Vol. XVII. p. 267.)

An Account of Experiments on the Velocity of Sound, made in Holland, by Dr. G. Moll and Dr. A. Van Beek.

Two and thirty pages of tabular details contain the researches of these learned Dutchmen, on the above subject; these we shall neither insert nor abridge, but rest content with laying the following table before our readers, which shows the result of Experiments on the Velocity of Sound, as observed by different philosophers, reminding them that the French metre is equal to 39.37 English inches.

Names of Observers.	Time when made.	Country where made.	Length of basis. Metres.	Velocity of Sound per Second in metres.	
Mersenne		France		448	1
Florentine Philosophers	1660	Italy	1800	361	2
Walker	1698	England	800	398	3
Cassini, Huigens, &c.		France	2105	351	4
Flamstead and Halley		England	5000	348	5
Derham	1704 and 1705	England	1600 à 2000	348	6
French Academicians	1738	France	22913 and 28526	332,93 at 0° *c*	7
Bianconi	1740	Italy	24000	318	8
La Condamine	1740	Quito	20543	339	9
La Condamine	1744	Cayenne	39429	358	10
T. F. Mayer	1778	Germany	1040	336,86	11
G. E. Muller	1791	Germany	2600	338	12
Epinoza and Banza	1794	Chili	16345	356,14 at 0°	13
Benzenberg	1809	Germany	9072	333,07 at 0°	14
Arago, Mathieu Prony	1822	France	18612	331,05 at 0°	15
Moll, Van Beck, and Kuytenbrouwer.	1823	Netherlands	17669,28	332,05 at 0° and dry air.	16

1. Mersenne de Aste Ballistica Prop. 39.
2. Tentamina Experim. Acad. del Cimento, L. B, 1738, Part II. p. 116.
3. Philos. Trans. 1698, No. 247.
4. Duhamel, Hist. Acad. Regi. L. II. Sect. 3, Cap. II.
5. Philos. Trans. 1708 and 1709.
6. Ibid, ibid.
7. Mem. de l'Academie des Sciences, 1738 and 1739.
8. Comment. Bononienses, Vol. II. p. 365.
9. La Condamine Introduction Historique, &c. 1751, p. 98.
10. Mem. de l'Acad. Royale des Sciences, 1745, p. 488.
11. J. T. Mayer, Praktische Géométrie Gottingen, 1792, B. 1. p. 166.
Muller, Gotting. Gelehrt. Anzeige, 1791, St. 159. and Voigt's Magazin, &c. B. 8, St. 1. p. 170.
13. Annales de Chimie et de Phys. T. VII. p. 93.
14 Gilbert's Annalen, neue Folge, B. V. p. 383.
15. Connaissance des Tems. 1825, p. 361.

A Catalogue of nearly all the principal Fixed Stars, between the Zenith of Cape Town, Cape of Good Hope, and the South Pole, reduced to the 1st of January, 1824. *By* the Rev. F. Fallows.

Remarks on the Parallax of α Lyræ, by J. Brinkley, D.D.

Of this, which is the concluding paper of the volume before us, we have already given a pretty copious abstract. [Vol. XVII. p. 264.]

Art. XIII. ASTRONOMICAL AND NAUTICAL COLLECTIONS.

No. XXI.

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i. *Remarks on the determination of the* Longitude, *from Observations of the Moon's Right Ascension. By* Thomas Henderson, *Esq.*

In the *Connaissance des Tems*, for 1825, p. 345, M. Bouvard gives a formula for the computation of the difference of longitude between two places, by means of the observed motion of the moon in right ascension, during the interval elapsed in passing from the one meridian to the other. It is this,

$$d = ar\left(\frac{15^\circ + m - h}{h}\right)$$

where a represents the observed motion of the moon in right ascension expressed in sidereal time, r the ratio of sidereal to mean time, m the mean horary motion of the sun, h the horary motion of the moon in right ascension for one hour mean time, and d the difference of longitude required.

It appears to me that this formula is erroneous, in so far as r is introduced; and that it ought to have been

$$d = a\left(\frac{15^\circ + m - h}{h}\right)$$

Which may be demonstrated thus:—

From the observed right ascension of the moon upon the meridian of the first place, calculate the mean time at Greenwich of the observation, and the right ascension of the meridian at Greenwich, by adding to the mean time there the sun's mean longitude at this time. The difference between the right ascension of the meridian at Greenwich, and the right ascension of the meridian at the place of observation, (being the same as the observed right

ascension of the moon,) is evidently the longitude of the first place from Greenwich.

In like manner let the longitude of the second place from Greenwich be calculated, and the difference between the two longitudes will be the difference of longitude required.

Comparing the two calculations, and employing the same notation as before, the difference between the mean times at Greenwich of the respective observations is =

$$\frac{a \times 15^\circ}{h}$$

The difference between the sun's mean longitudes at these times is $= \frac{am}{h}$.

And hence the difference between the right ascensions of the meridian at Greenwich at the respective times is =

$$a\left(\frac{15^\circ + m}{h}\right)$$

And the difference of longitude between the two places is =

$$a\left(\frac{15^\circ + m}{h}\right) - a = a\left(\frac{15^\circ + m - h}{h}\right)$$

An example will make this quite clear.

Let the moon's right ascension upon the meridian of the first place be $0^h\ 0'\ 0''$, and the sun's mean longitude the same. The moon will pass the meridian of the first place at $0^h\ 0'\ 0''$ mean time.

Suppose that the daily increase of the moon's right ascension is $1^h\ 0'\ 0''$, and of the sun's mean longitude $0^h\ 4'\ 0''$. Then the second place being supposed to be in $12^h\ 0'\ 0''$ of the west longitude from the first, the time of the moon passing the meridian of the second is thus found, by the rule given in the explanation of the *Nautical Almanac*.

At mean noon, under the meridian of the second place, the moon's right ascension is $0^h\ 30'\ 0''$, and the sun's mean longitude is $0^h\ 2'\ 0''$, the difference between which is $0^h\ 28'\ 0''$. Then as $24^h + 0^h\ 4'\ 0'' - 1^h\ 0'\ 0'' = 23^h\ 4'\ 0'' : 0^h\ 28'\ 0'' :: 1^h\ 0'\ 0'' - 0^h\ 4'\ 0'' = 0\ \ 56'\ 0'' : 0^h\ 1'\ 8''$. And $0^h\ 28'\ 0'' + 0^h\ 1'\ 8'' =$

$0^h\ 29'\ 8''$ mean time of the moon passing meridian of second place; at which time the sun's mean longitude will be found to be $0^h\ 2'\ 4.8''$, the moon's right ascension $0^h\ 31'\ 12''\cdot8$, the difference between which $0^h\ 29'\ 8''$ is the mean time of the moon's passing the meridian already found, and therefore confirms the calculation.

The increase of the moon's right ascension in passing between the two meridians is $31'\ 12''\cdot8$, from which M. Bouvard's formula makes the difference of longitude $11^h\ 58'\ 0''$, being $2'\ 0''$ erroneous, as the annexed calculation shows.

Log. of a, $31'\ 12'\cdot8$		3·2724914
Log. of r, $\dfrac{24^h}{24^h\ 4'}$		9·9987953
	$15^\circ\ 0'\ 0''$	
	$+m \quad +\ 2\ \ 30$	
	$-h \quad -37\ \ 30$	
Log of	14 25 0	4·7151674
A. C. Log. of h	0 37 30	6·6478175
Log. of d	11^h 58 0	4·6342716

The corrected formula is the same as M. Bouvard's, with the exception of r being expunged. It consequently makes the logarithm of d 4.6354763, and $d = 11^h\ 59'\ 59'.3$, which is as accurate as the data will admit of. The same result is obtained from the rules given by Professors Vince and Woodhouse, in their Treatises on Astronomy.

It therefore follows that M. Bouvard's calculations of the difference of longitude between Greenwich and Paris must be corrected, by adding to each result $1''\cdot53$, the amount of the error occasioned by his introducing r into the calculations.

It should be attended to, that different observers and different instruments do not always give the same measurements of the moon's diameter, and consequently that the mean longitude deduced from a number of observations of one limb of the moon, may differ from that deduced from observations of the other. The mean result of the whole observations, taken together, will be somewhat affected by the error arising from this cause, unless an equal number of observations of each limb has been made. When this has

not been done, the average result from the observations of each limb should be taken, and the mean of these held as the true longitude deduced from the whole observations.

Upon these principles, M. Bouvard's calculations of the difference of longitude between Greenwich and Paris will stand thus:—

From 31st August, 1800, to 6th August, 1803, when the observations both at Greenwich and Paris were made with the old transit instruments, those at Greenwich being by Dr. Maskelyne, and his assistants;

Mean of observations of first limb . .	9′ 24″·75	
Mean of observations of second limb	9 20 ·24	
Mean of both		9′ 22″·50

From 7th September, 1803, to 11th April, 1811, when the observations at Greenwich were made with the old transit instrument by Dr. Maskelyne, and his assistants, and those at Paris with the new transit instrument;

Mean of first limb	9′ 25″·85	
Mean of second limb	9 18 ·07	
Mean of both		9′ 21″·96

From 14th May, 1811, to 2d June, 1816, when the observations at Greenwich were made with the old transit instrument by Mr. Pond, and his assistants, and those at Paris with the new transit instrument;

Mean of first limb	9′ 20″·10	
Mean of second limb . . .	9 20 ·36	
Mean of both		9′ 20″·23

From 5th July, 1816, to 1st December, 1819, when the observations at Greenwich were made with the new transit instrument, and those at Paris, as formerly;

Mean of first limb	9′ 21″·43	
Mean of second limb	9 20 ·50	
Mean of both		9′ 20″·96

The formula $d = a\left(\frac{15^\circ + m - h}{h}\right)$ may be expressed in another manner, which will be found more convenient for calculation,

Let $R = \frac{15^\circ + m}{15} =$ be the ratio of mean time to sidereal, the logarithm of which is 0·0011873; then by eliminating the value of m from this expression, and substituting it, we obtain

$$d = aR\,\frac{15^\circ}{h} - a.$$

The multiplier $\frac{15^\circ}{h}$ expresses the ratio of one hour to the moon's motion in right ascension during that period, which is evidently the same as the ratio of any given period of time to the moon's motion in right ascension during the same period. Now if the moon's motion in right ascension during twelve hours in degrees, minutes, and seconds, be depressed a denomination, and reduced to minutes, seconds, and thirds, this will be the moon's motion in right ascension in time for three hours, the logarithm of the ratio of the latter of which to the former, and consequently of $\frac{15^\circ}{h}$ is equal to the proportional logarithm of the motion in time for three hours. And thus the logarithm of $\frac{15^\circ}{h}$ is obtained by inspection.

The above formula $d = aR\,\frac{15^\circ}{h} - a$, is adapted to mean time, but it will equally apply for apparent time, provided R be held to express the ratio of apparent time to sidereal, and the logarithm of $\frac{15^\circ}{h}$ be taken equal to the proportional logarithm of the moon's motion in right ascension in time, during three hours apparent time. The value of R will vary with the sun's motion in right ascension. A table of the logarithms of R is subjoined, in which the argument is the sun's motion in right ascension in time for twenty four hours apparent time. This manner of applying the formula is more convenient for the use of the *Nautical Almanac.*

The moon's motion in right ascension is always supposed to be that answering to the middle point of time between her passages over the respective meridians.

The following is an example of the last modification of the formula.

M. Bouvard makes the increase of the moon's right ascension, during her passage from the meridian of Paris to that of Greenwich, on 17th January, 1818, to be 20″·07 in time. Now the increase of the sun's right ascension in twenty four hours apparent time at that time was 4′ 16″·1; the middle point of time between the moon's passages over the respective meridians was 8^h 22′ apparent time at Paris, when the moon's motion in right ascension for twelve hours apparent time was (by the *Connaissance des Tems,*) 6° 13′ 44″, which depressed a denomination becomes 6′ 13″ 44‴, the moon's motion in right ascension in time for three hours.

Log. of 20″·07	1·30255
Log. of R (argument 4′ 16″·1) . .	0·00128
Prop. log. of 6′ 13″ 44‴ . . .	1·46085
Log. of 9′ 41″·6	2·76468
Subtract 20·07	
Remains longitude 9 21·5	

M. Bouvard makes the longitude 9′ 20″·0 from the observation; but the correction 1″·5 being applied, it becomes 9′ 21″·5, as above.

TABLE OF THE LOGARITHMS OF THE RATES OF APPARENT TIME TO SIDEREAL.

Argument.—Sun's motion in Right Ascension in time for 24 hours apparent time.

Arg.	Log.	Arg.	Log.	Arg.	Log.
3′ 30″	0·00105	3′ 40″	0·00110	3′ 50″	0·00115
3 31	0·00106	3 41	0·00111	3 51	0·00116
3 32	0·00106	3 42	0·00111	3 52	0·00116
3 33	0·00107	3 43	0·00112	3 53	0·00117
3 34	0·00107	3 44	0·00112	3 54	0·00117
3 35	0·00108	3 45	0·00113	3 55	0·00118
3 36	0·00108	3 46	0·00113	3 56	0·00118
3 37	0·00109	3 47	0·00114	3 57	0·00119
3 38	0·00109	3 48	0·00114	3 58	0·00119
3 39	0·00110	3 49	0·00115	3 59	0·00120

Arg.	Log.	Arg.	Log.	Arg.	Log.
4′ 0″	0·00120	4 10″	0·00125	4′ 20″	0·00130
4 1	0·00121	4 11	0·00126	4 21	0·00131
4 2	0·00121	4 12	0·00126	4 22	0·00131
4 3	0·00122	4 13	0·00127	4 23	0·00132
4 4	0·00122	4 14	0·00127	4 24	0·00132
4 5	0·00123	4 15	0·00128	4 25	0·00133
4 6	0·00123	4 16	0·00128	4 26	0·00134
4 7	0·00124	4 17	0·00129	4 27	0·00134
4 8	0·00124	4 18	0·00129	4 28	0·00135
4 9	0·00125	4 19	0·00130	4 29	0·00135

The following is an example of a calculation of the longitude from an observed right ascension of the moon upon the meridian, compared with the ephemeris, by the rule given in the beginning of this paper.

On 15th April, 1818, the right ascension of the moon's first limb, when upon the meridian of Greenwich, was observed = 142° 11′ 48″

Semidiameter in right ascension . .	16 32
Right ascension of moon's centre . .	142 28 20

From the *Connaissance des Tems*, we have ☽ A. R.

1818	° ′ ″		′ ″	′ ″
April 14, Midn.	131 35 17			
		6 31 54		
15, Noon	138 7 11		4 45	
		6 27 9		4 38
Midn.	144 34 20		4 32	
		6 22 37		
16, Noon	150 56 57			

	142 28 20
	138 7 11
As. 6° 27′ 9″ :	4 21 9 :: 12^h 0′ 0″
A. C. Log.	6 27 9 = 5·633970
Log.	4 21 9 = 4·195041
Log.	12 0 0 = 4·635484
	4·464495 = Log. of

Approximate time at Paris	8^h 5′ 40″
Equation of second difference + 30″·5 in time =	57
Apparent time Paris =	8 4 43
Sun's right ascension then =	1 33 29·7
Right ascension of meridian at Paris =	9 38 12·7
Observed R. A. of mer. at Greenwich 142° 11′ 48″ =	9 28 47·2
Longitude . . .	9 25·5

ii. *Discordance of the Lunar Observations made at* GREENWICH, *and at* PARIS. *In a Letter from* THOMAS HENDERSON, *Esq.*

SIR,

In making some calculations from the observations made at the Royal Observatories of Greenwich and Paris, I have been puzzled by a circumstance, which I do not observe has been yet taken notice of, but which seems to be highly deserving the attention of Astronomers. It is this. From 1800 to 1809, the observations made with the transit instrument at Greenwich show the sun and moon to be further advanced in right ascension, than those made with the transit instrument at Paris. From a mean of a great many observations, the difference appears to be about 3 or 4 seconds of space. From the Introduction to Burg's Lunar Tables, I observe that the epoch of the moon's mean longitude for 1801 is 3½ seconds greater by the Greenwich observations, than by those at Paris.

I find a similar difference, upon comparing the observations of the sun and moon's right ascensions, made with the transit instrument and mural circle at Greenwich, in the year 1812, the right ascension of these bodies by the transit instrument being about 4 seconds of space less than by the circle.

This discrepancy appears to be occasioned by some difference in the methods of observing the transits of the sun and moon, and of the stars, by the respective instruments. Should its cause be found out and obviated, the accuracy of the observations of the sun and moon would evidently be much improved. To accomplish this, the fittest course appears to be, to call the attention of astronomers to the fact, through the medium of your valuable Astronomical and Nautical Collections.

It is owing to this circumstance, that the difference of longitude between Greenwich and Paris, deduced from the moon's meridian right ascensions, is greater than the true difference. See my paper on this subject which I formerly transmitted to you.

I am, very respectfully,

Sir,

Your very obedient humble servant,

THOMAS HENDERSON.

Edinburgh,
31*st Dec.* 1824.

iii. *A Rule for clearing the* LUNAR DISTANCE *from the effects of Parallax and Refraction. By* CHARLES BLACKBURNE, *Esq.*

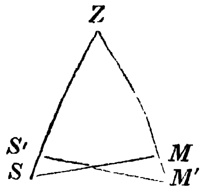
Let S and S' denote the true and apparent altitudes of ☉ or ✱, M and M', the true and apparent altitudes of the ☽, d the difference between the true altitudes, d' the difference between the apparent altitudes, Δ the apparent distance of the centres. Then,

RULE,

Add together the five following logarithms, *viz.* l. sin.$\frac{1}{2}\,\overline{\Delta + d'}$; the l. sin. $\frac{1}{2}\,.\,\overline{\Delta \backsim d'}$; the l. cosin. M; the l. sec. M'; the constant logarithm 0.301150, and reject 30 from the index.*

To the natural number belonging to this logarithm, add the v. sin. d; the result will be the v. sin. of the true distance†.

DEMONSTRATION.



Let ZS, ZS', be the true and apparent zenith distances of the sun or a star; ZM, ZM', those of the moon; then SM will be the true, and $S'M'$ the apparent distance of their centres. And by the principles of trigonometry,

$$\text{sin.}^2\tfrac{1}{2}Z = \frac{R^2 \times \text{sin.}\,\frac{1}{2}\,(\overline{ZS' \backsim ZM'} + S'M') \times \text{sin.}\,\frac{1}{2}(\overline{ZS' \backsim ZM'} \backsim SM')}{\text{sin.}\,ZS' \times \text{sin.}\,ZM'}$$

$$\text{or sin.}^2\,\tfrac{1}{2}\,Z = \frac{R^2 \times \text{sin.}\,\frac{1}{2}.\,\overline{\Delta + d'} \times \text{sin.}\,\frac{1}{2}\,\overline{\Delta \backsim d'}}{\text{sin.}\,S' \times \text{cos.}\,M'}$$

Again,

$$\text{cos.}\,SM = \text{cos.}\,\overline{ZS \backsim ZM} - \text{sin.}\,ZS \times \text{sin.}\,ZM \times \text{sin.}^2\,\tfrac{1}{2}\,Z \times \frac{2}{R^3}$$

$$\text{or cos.}\,SM = \text{cos}\,d - \text{cos.}\,S \times \text{cos.}\,M \times \text{sin.}^2\,\tfrac{1}{2}\,Z \times \frac{2}{R^3}$$

* The constant logarithm 0.301150 must be corrected, if necessary, by Table X. or XI. in page 33 of the Requisite Tables.

† If the index to the logarithm be 9, find the natural number to as many places as the tables are calculated to; if it be 10, to one more; if it be 8, to one less; and so on.

The rule requires no distinction of cases.

and therefore by substituting in this equation the former value of $\sin.^2 \frac{1}{2} Z$, we get

$$\cos. SM = \cos. d - \frac{\sin.\frac{1}{2}\overline{\Delta + d'} \times \sin.\frac{1}{2}\overline{\Delta - d'} \times \cos. S \times \cos. M \times 2}{\cos. S' \times \cos. M' \times R}$$

or $\cos. SM = \cos. d - \sin. \frac{1}{2} . \overline{\Delta + d'} \times \sin. \frac{1}{2} \overline{\Delta - d'} \times \cos. M \times \sec. M' \times \frac{2 . \cos. S}{R^3 . \cos. S'}$

or $\text{v. sin.} SM = \text{v. sin.} d + \sin. \frac{1}{2} \overline{\Delta + d'} \times \sin. \frac{1}{2} \overline{\Delta - d'} \times \cos. M \times \sec. M' \times \frac{2 . \cos. S}{R^3 . \cos. S'}$, which is the same as the Rule.*

Example.

Let the apparent distance of the moon from a star be 51° 28′ 35″; the apparent altitude of the star 24° 48′; that of the moon 12° 30′; and the hor. parallax 56′ 15″. Required the true distance.

	°	′	″
Δ =	51	28	35
d' =	12	18	0
	63	46	35
	39	10	35

l. sin. $\frac{1}{2} \overline{\Delta + d'}$ =	31 53 17·5	= 9·722851
l. sin. $\frac{1}{2} \overline{\Delta - d'}$ =	19 35 17·5	= 9·525379
l. cosin. M =	13 20 42	= 9·988112
l. secant M′ =	12 30 0	= 0·010418
the constant logarithm		0·301149
N =	353109 . .	9·547909
v. s. d =	19882	
v. s. . .	372991	51° 10′ 13″ = true dist.

iv. *Catalogue of the Stars in a Southern Constellation, denominated the* COMET *of* ENCKE. *By Mr.* CHARLES RUMKER.

The positions are deduced from Mr. Rumker's own observations. The clements of Aberration and Nutation are obtained according to the method of Zach, *Tables Nouvelles*.

* The logarithm 0·301150 is the logairthm of the constant quantity $2 \times \frac{\cos. S}{\cos. S'}$.

Name of Star.	AR. media pro initio 1823	Præc. in AR	Dec. med. pro initio 1823	Præc. in Dec.	Aberratio in AR. S. G. M.	Aberratio in Dec. S. G. M.	Nutatio in AR. S. G. M.	Nutatio in Dec. S. G. M.
a Enckii Cometæ	92 46 33.4 3 observ.	52.024	17 50 12.9 4 observ.	−0.966	5 27 27 1.32783	9 8 36 0.28810	5 20 32 1.27504	2 27 58 0.98424
b	93 42 24.19 4 observ.	52.038	17 4 54.8 4 observ.	−1.291	5 26 36 1.32590	9 9 38 0.36039	5 29 25 1.27647	2 27 16 0.98484
c	94 34 8.36 4 observ.	51.750	16 20 30.91	−1.593	5 25 27 1.32229	9 9 30 0.43044	5 29 19 1.2779	2 26 34 0.98384
d	93 12 14.73 5 observ.	51.403	15 27 35.28 5 observ.	−2.219	5 24 18 1.31863	9 9 23 0.50050	5 29 7 1.26839	2 25 3 0.98304
f	97 27 26.05 5 observ.	50.531	13 8 4.62 5 observ.	−2.600	5 23 9 1.31493	9 9 15 0.57056	5 28 56 1.25889	2 23 32 0.98225
g	98 50 14.14 5 observ.	50.496	13 4 44.74 5 observ.	−3.417	5 21 53 1.31714	9 10 44 0.57593	5 28 55 1.26091	2 22 7 0.98121
h	99 47 23.55	49.972	1 observ.					
i	100 32 11.72 2 observ.	49.718	11 0 00.30 2 observ.	−3.664	5 20 35 1.31380	9 8 54 0.64464	5 28 55 1.25714	2 22 7 0.98121
n	101 32 8.3 2 observ.	49.039	9 5 41.64 1 observ.	−4.001	id.	id.	id.	id.
o	101 38 33.05 4 observ.	48.993	8 57 57.32 4 observ.	−4.040	5 19 17 1.31046	9 7 5 0.71335	5 29 2 1.24949	2 21 7 0.9803
p	102 16 0.89 4 observ.	48.596	7 51 8.02 3 observ.	−4.257	5 18 41 1.3095	9 6 42 0.7354	5 29 2 1.24060	2 20 49 0.98000
s	103 12 40.49	1 obs.						
w	103 48 38.1 2 observ.	48.006	6 10 54.66 2 observ.	−4.779	5 17 34 1.30756	9 4 55 0.77947	5 29 10 1.23801	2 19 41 0.97898
x	104 7 42.13 2 observ.	47.990	6 9 6.03 2 observ.	−4.885	5 17 34 1.30756	9 4 55 0.77947	5 29 10 1.23801	2 19 41 0.9790
y	104 4 39.57	47.894	1 observ.					
β	104 47 56.5 2 observ.	47.479	2 observ.					
γ	104 49 3 75 5 observ.	47.405	4 27 0.21 6 observ.	−5.123	5 16.22 1.30556	9 3 29 0.82355	5 29 22 1.23761	2 18 57 0.97813
ε	110 5 23.82 7 observ.	45.392	1 32 45.01A 8 observ.	+6.871	5 11 27 1.30258	2 13 45 0.93112	5 29 42 1.22713	2 14 50 0.97282
ζ	111 46 29.64 2 observ.	44.932						
η	111 47 59.27 3 observ.	44.998	2 46 15.42 3 observ.	+7.431	5 10 44 1.30295	2 14 55 0.94733	5 29 25 1.22186	2 11 16 0.97191
θ	112 31 11.56 1 observ.							

Name of Star.	AR. media pro initio 1823	Præc. in AR.	Dec. med. pro initio 1823	Præc. in Dec.	Aberratio		Nutatio	
					in AR. S. G. M.	in Dec. S. G. M.	in AR. S. G. M.	in Dec. S. G. M.
κ Enckii Cometæ	115 25 0.87 7 observ.	43.119	8 44 30.29 7 observ.	+8.593	5 6 26 1.30517	2 21 50 1.04458	5 27 41 1.19023	2 10 51 0.96644
λ	115 42 39.21 3 observ.	43.055	8 57 29.4 2 observ.	+8.683		*Stargard, New South Wales, 12th Oct.* 1823.		
μ	115 52 19.28 2 observ.	42.979	9 12 25.94 1 observ.	+8.735				

v. *Remarks on* Hones. *Addressed to the Editor by a Correspondent.*

Sir,

An error in a *Table of Logarithms* may, indeed, chance to cause some trouble to a nautical and astronomical computer, once or twice in the course of twenty years; but a *bad razor* is a daily annoyance to seamen, as well as landsmen, to astronomers as well as gastronomers; and I therefore think it right to communicate to you some observations which have saved me a minute or two on an average every day of my life, for the last two or three years, and have spared me, perhaps, 100 out of 500 alternate motions of the hand and arm.

I have long been in the habit of using soap and water with a hone, in preference to the oil which is commonly employed; though I still doubt whether a constant immersion in oil might not, after all, keep the hone in the best possible order. But in any way that the hone is kept dry, its surface seems, in the course of a few months' use, to become hard or clogged, so that it will cut but very slowly, notwithstanding it may, at first, have possessed the requisite perfection of being soft enough to be scratched by a pin.

The remedy is to give it, every two or three months, a *new surface*, by a piece of common coarse *whetstone*, such as is used for scythes; and immediately after this *renovation*, a razor may be set in one minute as effectually as in ten by the same hone before the operation.

I am, Sir,

Your very obedient servant,

London, Jan. 1, 1825. Misopogon.

vi. *Further Remarks on* LUNAR TRANSITS, *in illustration of* MR. HENDERSON'S *Paper.*

1. It must first be observed, that when a difference of longitude is expressed in time, the time intended is *sidereal,* and not solar.

2. Thus the difference of longitude is equivalent to the difference of *absolute* sidereal time at which the same star comes to the meridian of two places.

3. But the *local* sidereal time of the transit of any star is the same at all places; while the difference of the local sidereal time of the transits of two stars, whether at the same place or at different places, gives the difference of their right ascensions; and the same is true of the moon and a star, or of the moon at two places.

4. Let this difference of the moon's transit, in sidereal hours, be called a; it will be expressed in degrees of space by $15^\circ a$, which will be the difference of the moon's right ascensions; and if the change of the moon's right ascension in an hour of solar time be h, and in an hour of sidereal time ϱh, the sidereal time required for this change will be $\frac{15^\circ a}{\varrho h}$, ϱ being $= \frac{365.24}{366.24}$.

5. In this difference of absolute time will be comprehended the difference of longitude d, and the additional difference of local time a, so that $\frac{15^\circ a}{\varrho h}$ will be equal to $d + a$, and $d = \frac{15^\circ a}{\varrho h} - a$; which agrees with Mr. Henderson's equation $d = aR\,\frac{15^\circ}{h} - a$. If we employ the Nautical Almanac, where the moon's place is computed for apparent time, Mr. Henderson's Table may often be of use in expediting the calculation. Mr. Bouvard seems to have expressed the difference of longitude in solar time, instead of sidereal, since $9^m\ 20^s$ of solar time are equal to $9^m\ 21.5^s$ of sidereal, answering to $2^\circ\ 20'\ 22''$ of space.

6. With respect to the diversity noticed by Mr. Henderson, between the right ascensions deduced from the observations at Greenwich, made with the transit instrument and the mural circle, it must be remarked, that the Astronomer Royal does not attach the slightest

weight to the latter as accurate records of the transits of the celestial bodies; and that they are noted for some subordinate purposes only; so that the transit instrument is the only authority that bears on this question.

vii. *Second Memoir on the Theory of Magnetism.* *By* Mr. Poisson. Read to the Royal Academy of Sciences, 27th Dec. 1824. From the *Annales de Chimie*, for January, 1825.

The former memoir on this subject, which I read to the Academy about a year ago, contains a detailed explanation of the principles which form the basis of the application of mathematical analysis to this important part of natural philosophy. These principles are hypotheses, to which we are led by the consideration of the most general facts in magnetism, and which are afterwards confirmed by the comparison of the results of calculation with those of experiments. The analogy which is observed between magnetic attraction and repulsion, and the mutual actions of electrified bodies, inclines us at first to attribute these phenomena, in the case of magnetism as well as in that of electricity, to two fluids, each of which attracts the particles of the other, and repels with the same force its own particles. But we soon discover that these imponderable fluids cannot be disposed, in bodies capable of being affected by magnetism, as they are in bodies which conduct electricity. In the latter, the two electrical fluids, as soon as they are separated from each other, tend to escape at the surface, and may pass, in any given quantity, from one body into another. But the case is different with respect to the *boreal* and *austral* fluids; for these fluids never quit even the smallest bodies to which they belong, however powerful the magnetizing forces may be: whence it has been inferred, that within the substance of bodies that are magnetized, the two magnetic fluids undergo displacements to an insensible distance only, which are nevertheless sufficient to render their action sensible without the body, being repulsive for the one, and attractive for the other.

I have given, in my former memoir, the name of *magnetical ele-*

ments of a body to the extremely small spaces, in comparison with its whole volume, through which the boreal and austral fluids can be separately moved; I have made no particular supposition with regard to their form, nor to their respective disposition; and I have considered them as insulated from each other by means of intervals impermeable to magnetism. According to this mode of considering the intimate nature of magnets, the sum of the magnetic elements which they contain, is a fraction of their volume, that may vary in different substances susceptible of being magnetic, and may also depend on their temperature; and in this manner we may explain how two bodies of the same form, but of different substances, or at different degrees of temperature, may exhibit, under the influence of the same forces, magnetical actions of very different intensities.

Beginning with this theory, which is here but briefly recapitulated, I have deduced from it, in the first memoir, the equations which express, for all possible cases, the laws of the distribution of magnetism within bodies that are rendered magnetical by induction, and those of the attractions or repulsions which they exert on points given in position. It is now only an analytical problem to resolve these equations, in order to deduce from them results which may be compared with experiment; but such a resolution is only attainable in a very limited number of cases, paying regard to the different forms of magnets. That which I have taken for an example in the first memoir, and which admits a complete resolution, is the case of a solid or a hollow sphere, magnetized by forces of which the centres of action are distributed in any given manner, either without or within it. If we reduce these forces to a single one, that is, to the magnetic action of the earth, the formulæ which contain the solution become very simple. We deduce from it without difficulty the direction of the needle of a compass, produced by the neighbourhood of a sphere, magnetized thus by the influence of the earth. This deviation varies with the distance of the middle of the needle from the centre of the sphere, from the plane of the magnetic meridian passing through that centre, and from the plane passing through the same point, perpendicularly to the direction

of terrestrial magnetism. The laws of these different variations, obtained by calculation, agree with those which Mr. Barlow, Professor at Woolwich, has deduced from a numerous series of experiments which he has made on the subject. The theory explains also a very remarkable fact observed by Mr. Barlow, relating to the magnetic action of a hollow sphere; he found that the intensity of this action does not sensibly vary with the thickness of the metal, at least when the thickness is not very inconsiderable, and is not less than about one thirtieth of an inch, for a sphere of ten English inches in diameter; whence he has inferred that the magnetism is confined to the surface of the magnetized bodies, or that it does not penetrate them beyond a very small depth. It appears, however, from the calculation, as founded on the distribution of two fluids throughout the mass of the magnets, that the action of a hollow sphere is very nearly independent of its thickness, as long as the proportion of this thickness to the radius is not expressed by a very small fraction, the value of which is different for different substances; a result which agrees perfectly with the experiment.

This remarkable agreement affords a very important confirmation of the accuracy of the analysis, and of the theory of magnetism on which it is founded. We might, however, be desirous of a still more diversified comparison of the theory with the phenomena; and for this purpose I have endeavoured to resolve the general equations of the first memoir, as applied to bodies not having a form so simple as that of the sphere. I have found, for example, that these equations may be resolved in a very simple manner for any elliptic spheroid, provided that the force which produces its magnetism be constant in magnitude and in direction throughout its extent; which happens with respect to the terrestrial magnetism. This solution is the object of the first paragraph of the memoir which I now offer to the Academy.

After having given the formulæ relating to a spheroid of which the axes have any imaginable relations to each other, I have particularly considered the two opposite cases of spheroids extremely flattened and extremely elongated. A spheroid greatly flattened

may represent a plate of which the thickness varies very slowly near the centre, and decreases from that point to the circumference. Its action on points near its centre must be sensibly the same as that of any other plate of a constant thickness, and of a very great extent. With regard to such a plate, I must here remark, that in the extract of my first memoir, the action of a plate of great extent has been erroneously compared with that of a hollow sphere, of which the ray is supposed to increase without limit; for the portion of the spherical surface most remote from the attracted point is as much greater in extent as the actions of all its points are weaker, so that its total action is a finite quantity, which is not to be neglected, as I had supposed. [Whether or no this correction was suggested by the doubt expressed in the translation inserted in these collections, the ingenious author has not thought it necessary to inform us; but the having been able to afford a useful hint, to a mathematician like Mr. Poisson, is an occurrence too flattering to the translator's vanity to allow him to pass it by in silence.]

In a similar manner we may assume that a spheroid greatly elongated approaches very near to the form of a needle or bar, of which the diameter decreases from the middle to the extremities, varying at first very slowly; and its action on points near its middle can differ but little from that of a bar, of which the diameter is constant, and very small in proportion to its length. When, therefore, we have experimentally observed the actions of a bar, or a plate, magnetized by the influence of the earth, on points very near the middle points of these bodies, we may compare our theory with observation in this new point of view. In order to facilitate this comparison, I have taken care to enunciate distinctly, in my memoir, the principal consequences of the calculation, which appear to be most deserving of experimental confirmation.

The second paragraph of the memoir relates to a question which is curious with regard to the theory, but still more important in a practical point of view, and which has lately excited much attention in England. I allude to the means of destroying the deviations to which the compass is subject on board of a ship, and which are occasioned by the magnetic action of the guns, the anchors, and

the other masses of iron by which it is surrounded. All these bodies are magnetized by the influence of the earth; in this state they act on the compass, and cause it to deviate sometimes very considerably, the deviations derived from this cause having amounted to 20° on either side of the natural direction of the needle, and even to near 40° in a voyage to high northern latitudes. They vary in the same place according to the situation of the vessel with regard to that direction; they vary also when the direction of terrestrial magnetism varies, with the latitude; so that it was desirable to find a method of destroying them applicable to all possible directions of the action of the earth, with regard to lines in certain fixed positions within the vessel. The method proposed by Mr. Barlow has been adopted in several voyages of considerable length; and if it has not entirely annihilated the deviations of the needle, it has at least confined them within narrow limits; and it has hitherto been judged sufficient for the occasions of navigation. It consists in placing near the compass a plate of iron, which is also magnetized by the action of the earth. It is placed in such a manner that the needle shall assume and preserve, in all the directions of the ship, a direction parallel to that of another horizontal compass, placed on shore, at a sufficient distance from the vessel to be out of the reach of its magnetic influence. Mr. Barlow supposes that, by trials, it will always be possible to find a position for the plate which will give it this property; and when it has been determined, he fixes the plate, and keeps it always in the same situation. If the other masses of iron contained in the vessel do not undergo considerable displacements, and if in reality the deviations have been annihilated in all directions at the point of departure, it is evident that they will continue to be neutralised throughout the voyage, notwithstanding the changes of the intensity and the direction of the terrestrial magnetism: and this is easily seen when we consider, on the one hand, that all the bodies, susceptible of being rendered magnetic, which form a part of the vessel, including Mr. Barlow's iron plate, being magnetized by the action of the earth, the intensities of their magnetic actions will increase or decrease in the same proportion as this force: and on the

other hand, that this force, remaining parallel to itself in the whole extent of the vessel, the change of its absolute direction can produce no other effect than the change of the situation of the vessel, with regard to this same direction. It must, however, be remarked, that we here suppose that the ship must be imagined to have been submitted, at the time that the adjustment is made, to a rotatory motion, not only in a horizontal plane round a vertical axis, but also in a vertical plane round a horizontal axis; not that the compass can ever be afterwards employed in such a situation, but since the inclination of the magnetic force may change without limit during the voyage, so that the relative change of direction is the same. [And it is unnecessary to remark, that such a previous experiment as this was never contemplated by the inventor of the apparatus.]

According to these considerations, the question, which is the subject of the last paragraph of the memoir, is reduced to the inquiry, whether it is possible to destroy identically, that is to say, for all possible directions of terrestrial magnetism, the deviations of a horizontal needle, derived from bodies magnetized by the influence of the earth, by adding to them a piece of iron, which is to be magnetized by the same cause. This requires, first, that the magnetism should have the same degree of mobility in the piece of iron, and in each of the other bodies; but we may admit that the coercive force may be very weak, so that the distribution of the magnetism may be always conformable to the actual direction of the action of the earth; a supposition which seems to be but little removed from the truth, with regard to the magnetic substances which are found on board of ships. With respect to the form of the bodies acting on the compass, I have supposed, in order to arrive at a complete solution of the proposed question, that all these bodies are spheres, either solid or hollow, of any given diameters and thicknesses, sufficiently distant from each other to allow us to neglect their mutual actions, and disposed in any assignable manner around the magnetic needle. Of such a system of spherical bodies, magnetized by the influence of the earth, I have determined the action on a given point, in order to see if, by properly fixing the

magnitude and the situation of one of them, one might not destroy the deviatory action of the whole system, with respect to a horizontal compass placed at that point; setting aside its reaction on the different bodies of the system.

The formulæ of my memoir show immediately that this action can never vanish for all the directions of the magnetizing force; consequently the duration of the oscillations of the needle will always be affected, even when there is no alteration in its direction. In order that the horizontal compass may be free from deviation, it is sufficient that the horizontal results of the action of the earth, and of the action of the system of magnetized bodies, should coincide with each other for all directions of the terrestrial magnetism. Now we find that this coincidence is only possible when a certain quantity, depending on the magnitude of the given spheres, on their mutual differences, and on their distances from the compass, and from the horizontal plane which contains it, is either positive or evanescent, and when another quantity, depending on the same elements, vanishes; and reciprocally, when these two conditions are fulfilled, we may produce the required coincidence, by adding a single sphere to the given system. Its magnitude, and the place of its centre, may still remain undetermined; but the direction of one or more right lines drawn through the centre of the compass, on which it must be placed, will be given; and its distance from the needle will depend on the diameter assigned to the sphere, and will be proportional to it. This latitude of the solution of the problem depends on the identity of the action of a sphere on a given point, while its centre moves on a line passing through the point, and at the same time its diameter increases or diminishes in the same proportion as the distance from the point. If the given system of spheres consists of a single one only, its centre, and that of the added sphere, must be situated in the horizontal plane which contains the needle, and the right lines drawn from the two centres, to its middle point, must be at right angles to each other; and it will be necessary that the lengths of the lines should be proportional to the diameters of the spheres; in this case the compass will constantly retain its natural direction, when the system of the two

spheres is made to turn round its point of suspension; which it would be easy to verify by experiment.

The example here considered, shows that *it is not always possible* to destroy in every direction the deviations of a magnetic needle, by *adding a new body* to the whole of those which have occasioned that deviation. Although we cannot assign, for bodies of all possible forms, such conditions as have been laid down for spherical bodies, we may at least determine, for all cases, the number of these conditions. When a system of bodies, magnetized by the action of the earth, and mutually influencing each other, affects the needle of a compass, it is necessary, in order that the horizontal direction of the needle may be constantly annihilated, that the system should correspond with equations belonging to the form and disposition of these bodies, which, in the most general extent of the problem, will be five in number. If among these bodies there is a sphere of given diameter, but indeterminate in its position, we may dispose of the three co-ordinates of its centre in such a manner, as to satisfy these five equations, and thus reduce them to two equations of condition; and, besides, there will be other conditions which must be fulfilled, in order that the values of the unknown quantities may be real. If the moveable body, instead of being a sphere, is, for example, a circular plate, of a given diameter and thickness, we may dispose of the three co-ordinates of its centre, and of the two angles which serve to determine its direction; we shall thus have as many indeterminate quantities as there are equations to be satisfied, and there will only remain the conditions necessary for the reality of the values of these five unknown quantities. The use which has been made of this method, on board of ships, having greatly diminished the deviations of the compass, it must be inferred that, in the ordinary disposition of the masses of iron which are present, the conditions relative to this system of bodies are nearly fulfilled; but *we can never be certain that other cases will not arise*, in which the addition of *a single body*, of determinate form and dimensions, *would not* be sufficient to destroy the deviations of the needle, or even to *reduce them to moderate limits*, especially if the dip should change very considerably in the course of the voyage.

Mr. Barlow has also proposed to employ his instrument in a manner which may be called the reverse of that which has been here described. Before the ship sails, he finds, by trial, a position of the plate fixed near the compass, such that, in all directions of the ship, it produces the same deviation with that of the ship itself, and in the same direction. Hence he infers, that if the deviations are not very great, the united actions of the iron and of the plate will occasion deviations about twice as great as those which either cause would produce alone. So that when the true variation is required in the course of the voyage, the observation being made twice in succession, once with and once without the plate, the difference of the two directions gives a measure of the effect; and we obtain the true natural variation, by adding this difference to the variation first observed, when the employment of the plate has diminished it, and by subtracting the difference, when the plate has increased the variation.

In order to form an estimate of the degree of generality of this mode of correction, I have inquired whether we could produce with a single sphere, for every direction of the earth's magnetism, the same deviations in a horizontal needle as are due to a system of spheres given in magnitude and in position, and magnetized, as well as the required sphere, by the influence of the earth. The investigation shows that this is only possible when the bodies fulfil, as in the former case of the destruction of their effect, two separate conditions; a certain quantity depending on the magnitudes and the dispositions of the given spheres must become evanescent, and another quantity which, in the former case, was to be positive or evanescent, must now be evanescent or negative. This method, therefore, is not more extensively applicable than the former; its application is less simple; and it becomes impracticable when the deviations are extremely great, though this is precisely the case in which the remedy is most wanted.

But when all the given spheres have their centres in the same horizontal plane which contains the compass, it is always possible to destroy their effect, or to imitate it by means of a single sphere placed in a proper situation. Its centre must be in the same plane with the rest, and we may find the directions of two right lines

meeting at right angles in the middle of the needle, on one of which the sphere must be placed, in order to destroy the effect of the other, and on the other, in order to imitate it; its distance from the compass depending, in both cases, on the magnitude which we may choose to assign to it.

I shall conclude this extract with the formulas relating to the action of a sphere magnetized by the influence of the earth, from which we may deduce, by very simple calculations, the results contained in the second paragraph of my memoir, which have already been explained.

Let a be the radius of the sphere; r the distance of its centre from the given point on which it acts; x, y, and z, the three co-ordinates of that centre, referred to three orthogonal axes, passing through the given point; α, β, and γ, the results of the action of the sphere reduced to those axes; and X, Y, and Z, those of the action of the earth expressed in the same manner; we shall then have:

$$X = \frac{-ka^3}{r^3}\left(\alpha - \frac{3\alpha x^2}{r^2} - \frac{3\beta yx}{r^2} - \frac{3\gamma zx}{r^2}\right)$$

$$Y = \frac{-ka^3}{r^3}\left(\beta - \frac{3\beta y^2}{r^2} - \frac{3\alpha xy}{r^2} - \frac{3\gamma zy}{r^2}\right)$$

$$Z = \frac{-ka^3}{r^3}\left(\gamma - \frac{3\gamma z^2}{r^2} - \frac{3\alpha xz}{r^2} - \frac{2\beta yz}{r^2}\right).$$

The quantity represented by k being a fraction depending on the nature of the sphere, and apparently differing little in general from unity. When the sphere is hollow, we must substitute for this quantity, $\frac{(a^3 - b^3)(1 + k)k}{(1+k)a^3 - 2k^2b^3}$, in which b is the interior semidiameter of the shell; this factor also differing very little from unity, except when the thickness, $a - b$, is extremely small in comparison with a.

In order to compute the deviation of a needle by these formulæ, we must consider its middle point as the beginning of the co-ordinates, and neglecting its length in comparison with r, the values of the forces X, Y, and Z, with regard to the two poles, may be considered as equal, and having contrary signs.

Art. XIV.—MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *Improvements in Microscopes.*—Dr. Goring has caused Mr. W. Tulley, of Islington, to execute a triple acromatic lens of .333 inches sidereal focus, and .2 inches aperture, and another of only .2 inches focus, and .11 inches aperture. Used as single lenses, these constitute the utmost perfection to which magnifying glasses can be brought by artificial combination, but their power is too low to be extensively efficient. Applied, however, as object glasses, to a compound microscope with various apertures, and eye glasses, according to the nature of the body under examination, they render the instrument *equal to single lenses of the same amplifying power* in its capacity of shewing the most difficult test objects,—a degree of superiority which no compound instruments in which a magnified image of an object is viewed, instead of the object itself, have ever yet attained *; and in consequence of which the luxurious accommodation of their large field of view, and the facilities they afford for illuminating opaque objects, have hitherto been justly rejected by the most distinguished naturalists in favour of the simple single lens. In justice to the celebrated Mr. Troughton, to whom science is so profoundly indebted on so many accounts, it is proper to observe, that he was the person at whose suggestion achromatic lenses were first made by Mr. Tulley, and which were intended to be applied as the object glasses of the microscopes of the Greenwich circle. The lenses in question were a little more than one inch focus, and a quarter of an inch aperture, but were rejected by Mr. Troughton, as no better than common ones, at least for the purpose they were intended for, and with the greatest

* Mr. Herschel, in a most original and masterly paper in the *Transactions of the Royal Society* for 1821, part 2, has ascertained the true theoretical curves for giving the smallest quantity of central spherical aberration in a magnifying glass composed of two lenses. Dr. Goring instigated Mr. Cornelius Varley to make that combination represented at fig. 5, in the plate attached to the paper alluded to, on such a scale as to give a focus of only 1-6th of an inch, with 1-15th inch of aperture. This forms the best object-glass for a compound microscope which can be made, (excepting the achromatus,) to which it approximates very much in point of distinctness; it however confuses the outline of delicate and minute transparent objects with a fringe of colour which is very prejudicial to vision. As a single microscope it performs admirably, and may probably be executed on a much smaller scale than 1-6th inch focus, so as to give a very useful power; as it is, it shews the texture of mother-of-pearl, and the fine lines or flutings on the feathers of moths and butterflies, and other test objects which require a power three or four times higher to be rendered visible in the *common* compound microscopes of commerce.

propriety; for though the chromatic aberration in them was in a great measure subdued, the spherical aberration was unaltered; their distinctness, therefore, was no greater than that of a common lens of the same angle of aperture. An immense difficulty remained to be overcome, *viz.*, the distinction of the aberration of figure, together with that arising from dispersion, which has now been effected. It is but a small point gained to render these lenses free from colour, for they may, notwithstanding, be no better, *or even a great deal worse*, with regard to distinctness, than common ones, as is the case also with the chromatic object glasses of telescopes.

Mr. J. Cuthbert has also (under the direction of Dr. Goring) constructed a reflecting microscope, on the principle of that invented by professor Amici, of Modena, which, *in its original condition and dimensions, viz., with an objective metal of $2\frac{1}{2}$ inches sidereal focus*, is good for nothing, notwithstanding the pompous eulogiums which have been bestowed on it; being unable to shew any but the most common and easy objects, as can be demonstrated by a very excellent one of the kind previously mad by Mr. Cuthbert, in which the objective metal has a truly elliptical figure, &c.* Dr. Goring was of opinion that the principle of the instrument was good, but that the failure in the performance arose from the object metal of $2\frac{1}{2}$ inches focus, with a tube 12 inches long, forming an image which was only about three times larger than the object, so that all the rest of the requisite power had to be obtained by very deep eye-glasses: he accordingly planned both the optical and mechanical arrangements for another instrument, which has an objective metal of only .6 inches sidereal focus, and .3 aperture, with a tube 5 inches long. This being executed by Mr. Cuthbert, was found to perform extremely well, and to exhibit any objects which could be seen with the single microscope, to which it seemed equal, power for power.

Dr Goring has also caused *a diamond lens* of $\frac{1}{20}$th inch focus, to be executed by Mr. Andrew Pritchard, of 51, Upper Thornhaugh Street, (assistant to Mr. Cornelius Varley, under whose auspices it was worked.) A diamond is well known to be the most refractory body in nature, at least it is the most difficult which could be selected to receive a spherical figure; yet it is possible to form a lens of it, as the event has shewn, and (if we neglect the obstacles which present themselves in working it) seems precisely the substance which is most adapted to form a small microscopic magnifier, for its refractive power is nearly double that of glass, while its dispersive power is no greater than that of water; its extreme hardness also ultimately causes it to receive the most exquisite figure and po-

* Mr. Dolland, it is said, has also executed one with the same results.

lish. Thus a diamond lens will always magnify very nearly twice as much as a glass one ground in the same tool, while its spherical and chromatic aberrations are no greater with a given aperture. The lens in question is plano-convex, and was ground in a tool which would have made a glass one of $\frac{1}{10}$th inch focus, to which it is precisely similar in size and outward figure: it carries the same aperture also equally well, (only with the peculiarity of magnifying twice as much, being very nearly $\frac{1}{20}$th inch focus.) Most unfortunately, several flaws have appeared in the stone, which is, moreover, at present imperfectly polished; it is nevertheless capable of acting very well, and shews the most difficult objects both as a single magnifier, and as the objective glass of a compound instrument; it has been used with as much as $\frac{1}{20}$th inch of aperture, and exhibited to many individuals who are perfect judges of these things. Mr. Cornelius Varley (who is exceeded by no man in his skill in working small lenses) proposes to make a lens of diamond in the same tool which would form a glass one of $\frac{1}{60}$th, inch focus, (being the smallest lens which can well be made,) which will, of course, have a focus of about $\frac{1}{100}$th of an inch. Farther particulars concerning the microscopes mentioned in this notice, may be had by application to the artists who have been here designated, and a full and particular account of each will be given in a work on the microscope, which is preparing for the press.

2. *Capillary Attraction.*—M. Gilleron says, " If a capillary tube be introduced into mercury, the metal will remain in the tube below the exterior surface. If then the tube be carefully raised, without taking it out of the mercury, the surface of the mercury in the tube may be raised to the level of that without. Operating very carefully, it may even be raised still higher; its surface will then become concave, the nature of the curve apparently approaching that of the catenarian curve, which I believe also to be that of liquids which in capillary tubes are raised above the level of the external surface. If then the tube be depressed a little, the convex surface may be again given to the mercury in the tube, without its level being depressed below that of the external portion. It appears to me, therefore, that the surface of liquids in capillary tubes is an accessory circumstance, and has no direct influence on the elevation or depression of the liquid."—*Bib. Univ.* xxvii. 209.

3. *Embossing on Wood.*—A new and ingenious method of embossing on wood has been invented by Mr. J. Straker. It may be used either by itself, or in aid of carving, and depends on the fact that if a depression be made by a blunt instrument on the surface of wood, such depressed part will again rise to its original level by subsequent immersion in water.

The wood to be ornamented, having first been worked to its pro-

posed shape, is in a state to receive the drawing of the pattern; this being put in, a blunt steel tool, or burnisher, or die, is to be applied successively to all those parts of the pattern intended to be in relief, and at the same time is to be driven very cautiously, without breaking the grain of the wood, till the depth of the depression is equal to the subsequent prominence of the figures. The ground is then to be reduced by planing or filing to the level of the depressed part; after which the piece of wood being placed in water either hot or cold, the parts previously depressed will rise to their former height, and will thus form an embossed pattern, which may be finished by the usual operation of carving.—*Trans. Soc. Arts.* xlii. 52.

4. *Drawing of Iron Wire facilitated.*—A manufacturer of iron and steel wire observed that the wire which had been pickled (a process requisite in the course of the drawing) in an acid liquor, the temperature of which had been raised by the immersion of some hot ingots of brass, passed through the holes in the drawing plates with remarkable facility, in consequence of the precipitation of a portion of the copper in the liquor upon its surface; it required to be annealed much less frequently than before, the copper apparently preventing the action of the plate so as to gall or fret the wire. In consequence of this fact, the same person has constantly availed himself since of the use of a weak solution of copper in iron and steel wire-drawing. The thin coat of copper is entirely removed in the last annealing process.—*Tech. Rep.* vii. 161.

5. *Hawkins' mode of preparing Emery.*—Mr. Hawkins finding that the emery sold in the shops was totally inefficient for the purpose he had in view, namely, grinding two flat surfaces of hard cast steel accurately, thought of applying a process he had seen for washing over *diamond dust*, to emery; and to be certain that his emery was of good quality, he purchased of an emery-maker a quantity of those small lumps, or grains, which had longest withstood the action of the cast-iron runners and bed, and thus ensured the hardness of the emery; these pieces were reduced to powder in a cast-iron mortar, and then separated into different portions by sieves.

He then washed over the finest emery thus obtained, using oil instead of water, as in the usual process, the former holding it in suspension for a much longer time; and in this way obtained a series of emery which had floated one, five, ten, fifteen, twenty, forty, and eighty minutes, amongst which he found every variety necessary for his purpose; and keeping them in boxes, which were numbered according to the minutes they had floated, he could at any time prepare more of any one kind. In this way Mr. Hawkins readily attained his object, and ultimately by selecting those grains of emery which resisted longest the action of the pestle and

mortar, he obtained some so hard, as to be capable of cutting a ruby, when employed instead of diamond-dust.

Mr. Gill by grinding Greek emery-stone between two flat and hard steel surfaces, and washing off the lighter portions in oil, found that those which subsided in half a minute, when examined by a microscope, had entirely resisted the friction, and were perfectly crystallized sapphires.—*Tech. Rep.* vii 45.

6. *Power of Building Materials to resist Frost.*—In the xvii. volume of this Journal, p. 148, we gave an account of a process devised by M. Brard, for the estimation of the power possessed by building materials, of resisting the disintegrating action of frost and weather. He imitated this action by the spontaneous crystallization of a solution of Glauber's salt, the crystals producing the same effect upon the particles of the stones submitted to trial as the formation of ice would have done by the exposure of the same stone to cold after being moistened. The importance of M. Brard's process, and its great utility to architects, has been proved in France by MM. Vicat, Billoudel, Conrad, and de Thury, who in consequence consider the means as known, by which the power of any building-stone or material to resist the disintegrating action of frost or weather, may be ascertained in a few days.

In addition to the directions formerly given, the following have been added in the instructions drawn up by the French philosophers. The stone having been selected, the cubes cut, and the solution of sulphate of soda (saturated at common temperature) made, the solution is to be boiled, and whilst boiling freely, the specimens are to be introduced. The stones are to be boiled for half an hour and not longer: M. Vicat having shewn that afterwards the effect surpasses that of frost. The specimens are then to be withdrawn one after the other, and suspended by threads, so that they do not touch each other, or any thing but the thread; a vessel containing some of the clean solution in which they have been boiled is to be placed beneath each, after which the vessel and its accompanying specimen are not to be separated.

If the weather be not too moist or too cold, the specimens will be found covered in 24 hours with small white saline needles. They are then to be plunged, each into the particular portion of solution beneath it, when the needles fall off, and are again to be suspended in the air. A repetition of this process is to take place each time the needles are well formed. If the stone under trial is capable of resisting the action of frost, the salt will remove nothing from it, and neither grains, nor scales, nor fragments, will be found at the bottom of the solution beneath. On the contrary, with a stone which gives way to the weather, it will be seen that even on the first day the salt will remove particles from it,

the cube will lose its angles and edges, and ultimately there will be found at the bottom of the vessel all that has been detached during the trial. The trial should be concluded at the end of the fifth day after the salt has first appeared in crystals. The formation of crystals may be facilitated by moistening the stone as soon as they have appeared on any one point; this may be repeated five or six times a-day.

Great care should be taken that the saturation of the water by the salt be effected at common temperatures. The experiments of M. Vicat and others have demonstrated, that stone, which resists perfectly the action of frost or of cold solution of sulphate of soda, gives way entirely when exposed to the action of a saturated hot solution: the same is the case frequently also, if the trial be continued beyond the fifth day. Mortars and bricks which had withstood ten winters gave way to saturated hot solutions; and M. de Thury found that lias and other stones which had resisted the weather for ages, were disintegrated by the same excessive kind of trial: from which it may be concluded, that stones which *can* resist these trials would scarcely undergo any change by exposure to weather for any length of time.

If it be required to estimate comparatively the power possessed by two or more kinds of stone to resist the action of frost; the portion of matter separated from them, and lying in the solution beneath, is to be collected, washed, dried, and weighed; and the weight indicates the proportion in which the samples tried would suffer by exposure to weather and frost.—*Ann. des Mines*, ix. 741.

7. *Economical Method of warming Manufactories*, &c.—Mr. Bewley, of Montraith, Ireland, has contrived a method of warming his cotton-mill by the waste heat of a lime-kiln, apparently in a very effectual manner. The kiln is closed in at the top by a cover of cast-iron, from the middle of which a cast-iron flue proceeds, carrying off the carbonic acid, smoke, *&c.* This is surrounded by a larger pipe intended for the conveyance of air, which commencing at a brick enclosure surrounding the top of the kiln, continues to the upper part of the building, and has openings from it into the rooms of the mill, for the passage outwards of the air that has been heated during its ascent through it. Altogether, therefore, it resembles in principle Perkins' stove. The particular advantages mentioned by Mr. Bewley are, that a very cheap kind of fuel may here be used, scarcely fitted indeed for any other purpose, namely, culm, cinders, *&c.*, and that the lime produced will in almost all situations pay the expenses, and in many afford a profit. The heat also is a very steady one, and is continued during the night as well as the day, by which changes of temperature, sometimes injurious, are avoided. The work-

people also are more willing to come to work, having warm workshops to come to. The method has the merit of being a very safe one.

The cotton-mill warmed contains five rooms, the four upper of which are supplied with warm air by the means described. The rooms are fifty feet long by twenty broad: the general temperature is said to be about 80° Fah. The kiln is called a small one, being eleven feet deep, seven feet greatest diameter. It is fed with limestone and fuel by a door in its cover, and is drawn twice in 24 hours. If a very steady heat be required, it is recommended to be drawn three or four times in 24 hours.

Where lime may not be wanted, the burning of bricks or tiles, or even clay for manure, is suggested, as admitting of the same arrangement.—*Trans. Soc. Arts*, xlii. 134.

8. *Method of consuming the Smoke of Steam-boiler Furnaces*, &c., by Mr. Chapman.—Mr. Chapman's process consists in the introduction of air into the furnace beyond the fire; but he uses peculiar means of heating this air before it is introduced, and this renders the combustion of the smoke more ready and perfect. He says, "To heat the air before its admission into the furnace. This I do by casting the grate-bars hollow from end to end, so that they form a series of parallel tubes, which open into two boxes, one placed in front, and the other behind the grate. In the front box, directly underneath the fire-door, I make a register to open and shut, to any extent, at pleasure. The other end I connect with the brick-work directly under the fire-bridge, which fire-bridge I make *double*, with a small interval between, say one inch; the interval to go across the furnace from side to side, and rather to incline forward, or toward the fire-door, so as to meet and reverberate the smoke on to the ignited fuel in the grate, which causes it to inflame and become a sheet of bright fire under the bottom of the boiler." Consequently when the register is open, air passes along the bars of the grate, becomes heated in its passage, and is then thrown on to the hot smoke, causing its inflammation and combustion.

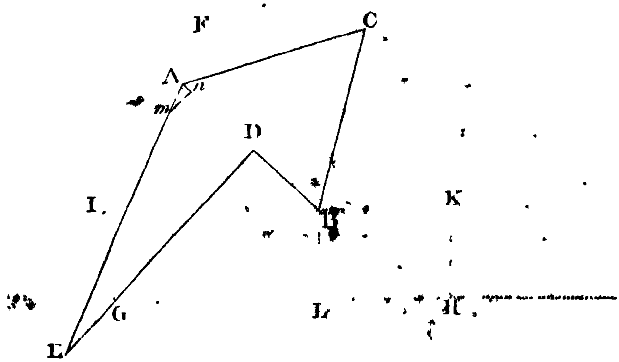
Mr. Chapman found that though his plan answered perfectly when the fire-door was closed, yet it was inefficient when the door was opened for the introduction of coals, so much cold air passing in as to prevent the due effect of the arrangement. To obviate this, he fed his fire with fuel in a different manner. "I adopted a cast-iron hopper above the fire-door, with a type at the bottom that has two pivots on one side, and opens at the other; one pivot goes through the end of the hopper, and has a counter-lever to keep the *type* shut, when a sufficient quantity of coal for a charge is on it. The top of the hopper is covered with a lid, which I shut down during the time of firing; then, by lifting the

lever which opens the type inside, the coals slide down on to the fore end of the grate bars, which is only the work of a moment. It is evident that no quantity of cold air can thus get into the furnace; in fact, it is not possible for any person that does not see the operation of firing, to know when fresh fuel is added by looking at the top of the chimney. The smoke that issues is never more than a light grey, just perceptible, but in a general way is not seen at all."

The coals last admitted are allowed to lie until partially coked, and just before a fresh supply is admitted, are pushed farther along the grate by a tool for the purpose, which remains constantly in the furnace: it is merely a plate of iron, about four inches broad, and as long as the grate is wide: it has a handle long enough to be used with both hands, which comes through a hole in the bottom of the door: when not in use it is drawn close up to the door. There is also another hole about one inch in diameter in the fire-door to look through; a circular piece of iron hangs before it by a loose rivet.

The certificates of persons who have witnessed the effect of this application speak to its entire success. Those who, previous to the erection of the engine, apprehended a nuisance, have been agreeably disappointed, smoke being seldom seen. On the application of fresh fuel, the smoke assumed the appearance of a light-grey vapour, which in a few seconds became almost invisible. On opening the fire-door in the usual way, dense black clouds of smoke rolled out of the chimney, but they ceased on closing the door again. An unlooked-for advantage also is, that the grate-bars appear to last longer when thus constructed.—*Trans. Soc. Arts*, xlii. 32.

9. *Graphical trisection of an Angle.*—The following instrument, for such it is, is supposed to be new, and may prove useful to artificers and others who have occasion to trisect any angle.



Having a plate of sheet metal, or other convenient material, let *A C B* be an equilateral triangle; divide one of its sides, A B, into two equal parts, as at D, through which draw the line C E, until it meets, as at E, another line A E, which latter makes an angle of 70° with A B; consequently, the angle E will be 20°. The periphery of the rectilineal figure, F A B C D, will represent the instrument in question.

To illustrate its application, suppose F G H the angle to be trisected; raise on one of its sides a perpendicular, as K H, equal to AD or B D, and through the point K draw I K, parrallel to G H; then place the instrument between this line I K, and the other leg F G of the angle given, so that the line A B touches these two lines at its extremities, whilst the line D E passes by the apex G of the same angle: when in this position, the angle A G D will be equal to one-third of the given angle F G H; for it is evident, that if from B the perpendicular B L be drawn to the line G H, and that the points B and G be joined by the line B G, that the rectangular triangles, L G B, D G B, A G D, will be equal, the sides, A D, D B, and B L being equal, and consequently, the opposite angles at G will also be equal.

The reason why the angle C is made equal to 60°, and the angle E to 20°, is as follows:—If the angle given was less than 60°, and consequently, its third smaller than the angle E, the perpendicular D E would, when employed as above, be too short to extend to G, and the operation, in this case, would be a little more complicated: instead of searching for the third of the angle given, the operation is to be commenced by finding the third of its supplement, which, once obtained, need only be subtracted from an angle of 60° to give the third of the angle required; now by making one of the angles of the instrument itself 60°, this graphical subtraction is very much facilitated, and the operation rendered so simple as to require no application. The line *m n* indicates a small triangle, *m* A *n*, which it is convenient to remove from the instrument, that the point A may have an acute angle instead of the obtuse angle C A E.—*Bib. Univ.* xxvii. 169.

10. *Secret Writing.*—Mr. Allsop describes, in a letter to the Editor of the Technical Repository, a mode of secret writing which he has long used for making private notes and memorandums. It is simply to substitute the preceding or succeeding letter for the one used; thus, for the letter *a* substitute *b*, for *b*, *c*, and so on. The following is a specimen:—

"Sir, Among the numberless inventions adopted for Secret
"Sir, Bnpoh uif ovncfsmftt jowfoujpot bepqufe gps Tfdsfu

Always addressing and concluding the letter in the usual manner, to prevent a discovery, or giving a key to the cipher.—*Tech. Rep.* vii. 174.

11. *Ink similar to China Ink.*—M. Fontenelle says, that an ink, equal in colour and goodness to China or Indian ink, may be made by dissolving six parts of isinglass in twelve of water, one part of Spanish liquorice in two of water, mixing the solutions whilst warm, and incorporating with them one part of the best ivory-black, using a spatula, and adding but small portions at once. When the mixture is complete, it is to be heated in a water bath, until so much water is evaporated as to leave a paste which may be moulded into any required form, and then the drying completed.

12. *English Opium.*—Messrs. Cowley and Stains still continue to grow poppies for opium*, and the following result will shew with what success this branch of agriculture is likely to be attended. In the year 1823, they collected as much as 196 lbs. of opium from poppies growing on twelve acres, one rod, and thirteen poles of land. Its character was such in the market, that it sold at two shillings per pound above the best foreign opium, and they believe that nothing but the carelessness of cultivators is likely to bring it into disrepute. One of the most positive directions given to those employed in collecting it, is to avoid the fall of petals, stamina, *&c.* into the receivers, and to take care if an implement falls to the ground, that it be properly cleaned from grit, *&c.*, a small quantity of which would seriously injure the quality.

The expenses attendant upon the cultivation of the twelve acres, one rod, and thirteen poles of white poppies, and the extraction of the opium, seed, and extract, amounted to 274*l*. 1*s*. 9*d*., of which above 103*l*. was paid to the labourers who collected the opium. The produce was as follows:—

	£.	*s.*	*d.*
Opium, 196 lbs., at 1*l*. 10*s*. 6*d*. per lb. .	298	18	0
Seed, 25 cwt. 1 qr. 22 lbs., at 12*s*. per cwt. .	15	5	3
Ditto unsold, about 5 cwt., worth . . .	3	0	0
Extract, 381 lbs., at 1*s*. 6*d*.	28	11	6
Turnips, 10 acres, at 2*l*. 10*s*. per acre . .	25	0	0
	370	14	9

There is one remark respecting the soil brought into this kind of cultivation, so important, that we quote it at length. "A

* Quarterly Journal of Science, xv. 139.

porous subsoil appears to us as a circumstance of the first-rate importance, for where it consists of clay, our crops have invariably been inferior to those which have grown on such parts as were situated upon the sand, although assisted with more manure; and this too, when, owing to frost, no injury could be attributable to the treading of the sheep, when feeding off the turnips. So strong, indeed, is our conviction of the ill effects of an impervious subsoil, that we have no hesitation in saying, that however good the soil, or however dry it may appear, if it be situated immediately above clay, no profit can be extracted from it by the growth of poppies, so frequent will be the partial (or total) failure of the crop."—*Trans. Soc. Arts*, xlii. 9.

13. *Letter to the Editor of the Quarterly Journal of Science, Literature, and the Arts, concerning* MR. JOB RIDER'S *rotatory Steam-Engine. By* ANDREW URE, M.D., F.R.S., *&c.*

DEAR SIR,—In your 16th volume, there are two letters descriptive of the structure and performance of the above engine. The first letter is signed Job Rider, the second William Boyd. These letters were transmitted to me from Belfast, in order that I might send you an account of the engine, for insertion in your Journal. I accordingly forwarded the letters, after making two or three merely verbal corrections, which certainly did not, in the slightest degree, alter the sense. For the truth of this assertion, you yourself can vouch, in regard to Mr. Boyd's letter, since it was from his own holograph, that pages 269 and 270 of the above volume of the Journal were printed*.

It now appears that Mr. Boyd did not intend that his letter should be published, but as he gave me no hint whatever to this effect, I did not feel myself justified, as a mere organ of transmission, in withholding that letter which alone furnished an account of the performance of the engine. Towards the conclusion of this letter, there is an observation which has given offence to Messrs. Girdwood and Co., eminent engineers in this city. The engine which this company made on Mr. Rider's plan for the Highland Lad steam-packet, was some time ago taken out of the boat, (in which it gave no satisfaction,) and has been since mounted in their manufactory. Here I have seen it in action. It was working the powerful blast bellows of their foundry, turning a loam-mill, and impelling the turning lathes, as well as the boring-machinery of two extensive floors of their workshop. I have therefore little doubt, both from this evidence, and the well-established reputation of Messrs. Girdwood and Co., that the rotatory

* The types were set up from Mr. Boyd's original letter, on which no alteration had been made in the least affecting the sense.—ED.

engine owed its failure in the steam-boat, not to bad workmanship on the part of the Glasgow engineers.

I am, dear Sir,

Yours very truly,

Glasgow, March 1st, 1825. ANDREW URE.

14. *On Paratonnerres, or Conductors of Lightning.*—A very interesting report on the subject of *Paratonnerres*, has been presented to the Royal Academy of Sciences by M. Gay-Lussac, in the name of a commission appointed specifically for the purpose, and an account of which has since been published in the *Annales de Chimie*, and more recently a translation has appeared in the *Annals of Philosophy*, for December, 1824. The paper is divided into two parts; one *theoretical*, and the other *practical*, and the information contained in it may be regarded as the most perfect we possess on the subject.

The theoretical part is introduced with some general observations on electric matter, and of conductors; that its velocity is at the rate of about 1950 feet per second; that it penetrates bodies, and traverses their substance, with unequal degrees of velocity; that the resistance of a conductor increases with its length, and may exceed that which would be offered by a worse but shorter conductor; and that conductors of small diameter conduct worse than those of larger. The electric matter also tends always to spread itself over conductors, and to assume a state of equilibrium in them, and becomes divided among them in proportion to their form, and principally to their extent of surface; and that hence a body that is charged with the fluid being in communication with the immense surface of the earth, will retain no sensible portion of it.

Gay-Lussac defines a paratonnerre to be a conductor which the electric matter prefers to the surrounding bodies, in order to reach the ground, and expand itself through it; and commonly consists of a bar of iron elevated on the buildings it is intended to protect, and descends without any *divisions* or *breaks* in its length, into *water* or *moist ground*. When a paratonnerre has any *breaks* in it, or is not in perfect communication with a moist soil, the lightning having struck it, flies from it to some neighbouring body, or divides itself between the two, in order to pass more rapidly into the earth.

The most advantageous form that can be given to the extremity of a paratonnerre is that of a very sharp cone, and *the higher it is elevated in the air*, other circumstances being equal, *the more its efficacy will be increased*, as is proved by the experiments of M.M. de Romas and Charles.

It has not been accurately ascertained how far the sphere of action of a paratonnerre extends, but in several instances, the more remote parts of large buildings on which they have been erected, have been struck by lightning at the distance of three or four times the length of the conductor from the rod. According, however, to the opinion of Charles, *a paratonnerre will effectually protect from lightning a circular space, whose radius is twice that of the height of the conductor.* By increasing, therefore, the altitude of a conductor, the space also which it will protect is augmented in proportion.

A current of electric matter, whether luminous or not, is always accompanied by heat, the intensity of which depends on the velocity of the current. This heat is sufficient to make a metallic wire red hot, or to fuse or disperse it, if sufficiently *thin*; and hence we may perceive the absurdity of some attempts which have been lately made, to protect ships, by *thin* slips of copper nailed to their masts. The heat of the electric fluid scarcely raises the temperature of a *bar* of metal, *on account of its large mass*; and no instance has yet occurred of an iron bar, of rather more than half an inch square, or of a cylinder of the same diameter, having been fused, or even heated red hot by lightning. A rod of this size would, therefore, be sufficient for a paratonnerre; but as its stem should rise from 15 to 30 feet *above* the building, it would not be of sufficient strength at the base to resist the action of the wind, unless it were made much thicker at that part. An iron bar, about three-quarters of an inch, is sufficient for the conductor of the paratonnerre.

According to Gay-Lussac, a paratonnerre consists of two parts, the *stem* which projects into the air above the roof, and the *conductor*, which descends from the foot of the stem to the ground. The stem he proposes to be a square bar of iron, tapering from its base to the summit, in form of a pyramid, and for a height of from 20 to 30 feet, which is the mean length of the stems placed on large buildings; the base should be about 2½ inches square. Iron being very liable to rust by action of air and moisture, the point of the stem would soon become blunt; and therefore, to prevent it, a portion of the top, about 20 inches in length, should be composed of a conical stem of brass or copper, gilt at its extremity, or terminated by a small platina needle, two inches long. Instead of the platina needle, one of standard silver may be substituted, composed of nine parts of silver, and one of copper. The platina needle should be soldered with a silver solder to the copper stem; and to prevent its separating from it, which might sometimes happen, notwithstanding the solder, it should be secured by a small collar of copper. The copper stem is united to the iron one, by means of a gudgeon, which screws into each; the gudgeon is first fixed in the copper stem by two steady pins at right angles

to each other, and is then screwed into the iron stem, and secured there also by a steady pin.

The conductor should be about three-quarters of an inch square, and as before stated, should reach from the foot of the stem to the ground. It should be firmly united to the stem, by being tightly jammed between the two ears of a collar, by means of a bolt. The conductor should be supported parallel to the roof, at about six inches distance from it, by forked stanchions, and after turning over the cornice of the building, without touching it, should be brought down the wall, and to which it should be fastened by means of cramps. At the bottom of the wall, it is bent at right angles, and carried in that direction 12 or 15 feet, when it turns down into a well.

Since iron buried in the ground in immediate contact with moist earth soon becomes covered with rust, and is by degrees destroyed, the conductor should be placed in a trough filled with charcoal, in the following manner. Having made a trench in the earth, about two feet deep, a row of bricks is laid on their broad faces, and on them others on edge; a stratum of bakers' ashes (*braise de boulanger*) is then strewed over the bottom bricks, about two inches thick, on which the conductor is laid, and the trough then filled up with more ashes, and closed by a row of bricks laid along the top. Iron thus buried in charcoal, will undergo no change in 30 years. After leaving the trough, the conductor passes through the side of the well before alluded to, and descends into the water to the depth of at least two feet, below the lowest water line. The extremity of the conductor usually terminates in two or three branches, to give a readier passage to the lightning into the water. If there be no well at hand, a hole must be made in the ground, with a six-inch auger, to the depth of about 10 or 15 feet, and the conductor passed to the bottom of it, placing it carefully in the centre of the hole, which is then to be filled up with bakers' ashes, rammed down as hard as possible, all round the conductor. In a dry soil, or on a rock, the trench to receive the conductor should be at least twice as long as that for a common soil, and even longer, if thereby it be possible to reach moist ground. Should the situation not admit of the trench being much increased in length, others, in a transverse direction, should be made, in which small bars of iron, surrounded by ashes are placed, and connected with the conductor. In general, the trench should be made in the dampest, and consequently lowest spot near the building, and the water gutters made to discharge their waters over it, so as to keep it always moist. *Too great precautions cannot be taken to give the lightning a ready passage to the ground, for it is chiefly on this, that the efficacy of a paratonnerre depends.*

As iron bars are difficult to bend according to the projections of a building, it has been proposed to substitute *metallic ropes* in

their stead. Fifteen iron wires are twisted together, to form one strand, and four of these form a rope, about an inch in diameter. To prevent its rusting, each strand is well tarred separately, and after they are twisted together, the whole rope is tarred over again with great care. Copper or brass wire is, however, a better material for their construction than iron. If a building contain any large masses of metal, as sheets of copper or lead on the roof, metal pipes and gutters, iron braces, *&c.*, they must all be connected with the paratonnerre, by iron bars of about half an inch square, or something less. Without this precaution, the lightning might strike from the conductor to the metal (especially if there should be any accidental break in the former), and occasion very serious injury to the building, and danger to its inhabitants.

Paratonnerres for Churches.

For a tower, the stem of the paratonnerre should rise from 15 to 24 feet, according to its area; the domes and steeples of churches, being usually much higher than the surrounding objects, do not require so high a conductor as buildings with extensive flat roofs. For the former, therefore, their stems, rising from three to six feet above the cross or weather-cock, will be sufficient, and being light they may easily be fixed to them without injuring their appearance, or interfering with the motion of the vane.

Paratonnerres for Powder-Magazines.

These require to be constructed with the greatest care. They should not be placed on the buildings, but on poles at from six to ten feet distance. The stems should be about seven feet long, and the poles of such a height, that the stem may rise from 15 to 20 feet above the top of the building. It is also advisable to have several paratonnerres round each magazine. If the magazine be in a tower, or other very lofty building, it may be sufficient to defend it by a double copper conductor, without any paratonnerre stem. As the influence of this conductor will not extend beyond the building, it cannot attract the lightning from a distance, and will yet protect the magazine, should it be struck.

Paratonnerres for Ships.

The stem of a paratonnerre for a ship, consists merely of a copper point, screwed on a round iron rod, entering the extremity of the top-gallant mast. An iron bar, connected with the foot of the round rod, descends down the pole, and is terminated by a crook or ring, to which the conductor of the paratonnerre is attached, which, in this case, is formed of a metallic rope, connected at its lower extremity with a bar or plate of metal, and which latter is connected to the copper sheathing on the bottom of the vessel.

Small vessels require only one paratonnerre; large ships should have one on the main-mast and another on the mizen-mast.

The late ingenious Mr. George Singer, in his excellent work on Electricity, proposed to have conductors *fixed to the surfaces of masts*, and the electric fluid conveyed by means of strips of metal over the deck and the sides of the vessel; but this arrangement on many accounts is highly objectionable, and the mode proposed by Gay-Lussac, or perhaps that commonly adopted in the British navy, of conveying the electric fluid from the mast head to the surface of the water, in a direct line, by means of a series of long copper links, is the best which has hitherto been devised.

It is allowed from experiment, that the stem of a paratonnerre effectually defends a circle of which it is the centre, and whose radius is twice its own height. According to this rule, a building 60 feet square, requires only a stem of 15 or 18 feet raised in the centre of the roof. A building of 120 feet by the same rule, would require a stem of 30 feet, and such are sometimes used; but it is better, instead of one st m of that length, to erect two of 15 or 18 feet, one placed at 30 feet from one end of the building, the other at the same distance from the other end, and consequently 60 feet from each other. The same rule should be followed for three or any greater number of paratonnerres. A plate is given in the *Annals of Philosophy* to illustrate this interesting subject more particularly.

15. *Influence of Copper, &c., on Magnetic Needles.*—Nov. 22, 1824. M. Arago communicated to the Academy of Sciences his experiments relative to the oscillations of a magnetic needle surrounded by different substances. He had ascertained that the copper rings with which dipping needles are generally surrounded exerted on the needles a very singular action, the effect of which was rapidly to diminish the amplitude of the oscillations without sensibly altering their duration. Thus when a horizontal needle suspended in a ring of wood by a thread without tension, was moved 45° from its natural position, and left to itself, it made 145 oscillations before the amplitude was reduced to 10°. In a ring of copper, the amplitude diminished so rapidly that the same needle, removed 45°, from its natural position, only oscillated 33 times before the arc was reduced to 10°. In another ring of copper, of less weight, the number of oscillations between the arcs of 45° and 10° were 66. The time of the oscillations appeared to be the same in all the rings.

In the ring of wood	145	oscillations from	45° to 10°.
copper	33	. . . .	45° „ 10°.
In a lighter copper ring	66	. . . .	45° „ 10°.

16. *Intensity of Electro-dynamic Force.*—M. Bequerel has ascertained, by experiments, that the intensity of the electro-dynamic

force in a metallic wire joining the two poles of a voltaic pile, is constant for all points of the wire.

II. Chemical Science.

1. *Variation of Boiling Points—Increased Production of Vapour.*—It has been known for some time that when certain kinds of extraneous substances are introduced into boiling fluids, considerable effect is produced upon the boiling point, vapour being formed either at lower points or with much increased facility. Thus Gay-Lussac has shewn that metal filings thrown into water, heated in a glass vessel, lowers the boiling point of the water 2° or 3°, and Mr. South pointed out the effect produced by putting platina wire or slips of platina foil into hot sulphuric acid, causing it to boil readily, quietly, and at lower points in glass vessels, than it otherwise would do, the difference here being several degrees.

Dr. Bostock has observed a remarkable fact of this kind in the extent to which the boiling point of ether may be changed by the introduction of a small chip of wood, or a portion of quill or feather of any kind. Ether, in a glass vessel, boiled freely at 112°, and with difficulty at 110°. Employing another glass vessel, it would not boil till the temperature had attained 150°, and the latter point was retained in other vessels. Repeating the experiment in a new vessel, it boiled earlier than before, but the vapour was observed to come off from one point where some substance had adhered to the glass. This led to the introduction of a small cedar chip, when the wood was quickly covered with bubbles, and the ether brought rapidly into ebullition. In this way ether boiled at 102°, which, without the wood, required 150°. The wood was not so effectual after some time as at first. When completely soaked with the ether it sunk to the bottom, and the ebullition nearly ceased; a fresh piece renewed it. Fragments of broken glass lowered the boiling point considerably. A small piece of metallic wire or copper filing, put into ether at 145°, caused a sudden and copious explosion of gas or vapour, and lowered the boiling point many degrees. Plunging a thermometer into the hot ether, caused production of bubbles at a temperature many degrees below the boiling point, no thermometer being present; after a time the effect ceased, but removal of the thermometer from the ether, and then re-immersion of it, produced a repetition of the effect. The cedar wood acted best when perfectly dry.

Alcohol of S. G. .848, boiled in a glass vessel at 182°, but by dropping in successive pieces of cedar wood the boiling point was reduced as much as 30° and 40°. The boiling point of water, Dr. Bostock found, was altered 4° or 5° by chips of cedar wood, requiring a temperature of about 217° when heated in a glass tube, by means of hot brine, but being brought down to the usual boiling point by the chips.—*Ann. Phil. N. S.* ix. 196.

2. *Oersted on Accelerating Distillation.*—In *Gehlen's Journal*, i. 277, I have related a few experiments which demonstrate that the disengagement of gas in a fluid resulting from chemical decomposition, never takes place, except in contact with some solid body. This principle may, without doubt, be applied to the disengagement of vapours. If a metallic wire be suspended in a boiling fluid, it instantly becomes covered with bubbles of vapour. Hence it might be concluded that a large number of metallic wires, introduced into a fluid which we wish to distil, would accelerate the formation of vapours. To prove this opinion, I introduced ten pounds of brass wire, of one-fifth of a line in diameter, loosely rolled up, into a distillatory vessel, containing 20 measures (about 10 pints) of brandy; the result was, that seven measures of brandy distilled over with a heat which, without the wire, was capable of sending over only four measures.

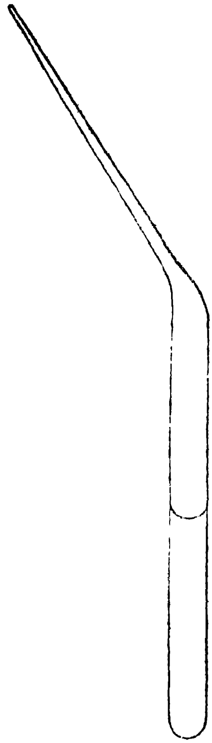
An expedient similar to this has been long in common use in England. When a steam-boiler has become incrusted with so much earthy matter that the contained water ceases to boil with rapidity, it is customary to throw in a quantity of the residue obtained from malt, by extracting its soluble portion, and which chiefly consists of small grains or fibres. Here the disengagement of vapour is promoted by the large number of thin and solid particles.—*Ann. Phil. N.S.* ix. 157.

The Editor of the *Annals* considers it probable that M. Oersted refers to the statement made by Mr. Bald in the *Edin. Phil. Jour.* ii. 340. That gentleman states that *cornings* are used for this purpose; they are the radicles of barley produced by malting, and separated before the malt is sent to market. About a bushel of these is thrown into the boiler, and when the steam is raised there is not only a plentiful supply to produce the full working speed of the engine, but an excess going waste at the safety valve. This singular effect will continue several days.

3. *Maximum Density of Water.*—Professor Hällostrõm, in a memoir which has appeared in the Swedish Transactions for 1823, deduces the temperature of the maximum density of water, as 39.394° Fahrenheit. Endeavours were made to estimate every cause which interfered with the experiments, such as dilatation of glass, *&c.*, and he thinks the limits of uncertainty are 0.428° Fahrenheit on either side of the above number.

4. *On the substitution of Tubes for Bottles, in the preservation of certain Fluids, such as Chloride of Sulphur, Protochlorides of Phosphorus, and Carbon,* &c.—There are many fluids in the laboratory, which are much more conveniently retained in tubes, such as that depicted in the margin, than in bottles, and from which they may be taken in a less wasteful manner when required for the purpose of experiment. A piece of glass tube, a quarter of an inch or

more in diameter, being selected, is to be closed at one end by the blow-pipe; and then, being softened near the other end, is to be drawn out obliquely, so as to form the long narrow neck represented in the wood-cut, but to which, in the first case, the short piece of tube is to be left attached; this forms a funnel, into which the preparation to be preserved is to be put. Then, warming the body of the tube, the expanding air passes out through the fluid; and, afterwards, on cooling the vessel, the liquid descends into it. A small spirit-lamp flame being now applied at the upper part of the long neck, softens the glass, which is then to be drawn out to a fine point and sealed. In this state the substance may be preserved clean and pure for any length of time.



If a small portion be required for an experiment, the extreme point of the neck is to be opened by pinching it off, the tube is then to be inclined until the quantity required has entered the neck, where, by capillary attraction, it will form a small column, and the tube being warmed by the hand, the atmosphere within it will expand and expel the portion of fluid on to the place required. A very little practice will enable the experimenter to judge of the quantity he is forcing out, and in this way he may take a portion not larger than the 1-20th of a common drop, or he may take the whole contents of the tube. When the quantity required has been taken out, the tube is to be placed in an upright position, and the flame of a lamp, or candle, or even a piece of paper, closes the aperture in a moment and as perfectly as before.

I have found these tubes very serviceable when working with substance either very small in quantity or obtained with great difficulty, in consequence of the entire prevention of waste resulting from their use. They are easily labelled by scratching the name of the substance with a diamond on them, and may conveniently be retained by putting several of them together into a tumbler, or other glass of that kind.—M. F.

5. *Supports for Substances before the Blow-pipe.*—Lieutenant-colonel Totten has adopted a modification of Mr. Smithson's contrivance. He pulverises a portion of the mineral to be tried, forms a paste of it with very thick gum-water, and rolling it under the finger moulds it into an acute cone, sometimes nearly an inch

long, and 1-20th of an inch in diameter at the base. These may be directed accurately upon the minutest visible particle, and being slightly moistened at the point with saliva, the particle will adhere to the apex under the strongest blast of the blow-pipe. These cones need not be more than one-fourth or one-fifth of an inch in length, for so effectually is the conducting power of the mass interfered with by the pulverization of the mineral, that one of them, half an inch in length, may be held in the fingers whilst the apex is in the focus of heat. A great advantage of this method over others is, that if fusion ensues, it is owing entirely to the nature of the substance experimented upon, and not to the agency of foreign substances acting as fluxes.—*Ann. Lyceum, of New York.*

6. *Examination of Fused Charcoal.*—At last a specimen of fused charcoal or supposed artificial diamond has been examined. The specimen was obtained by Professor Macneven of New York, by means of Hare's deflagrator, was sent to Dr. Cooper, and by him presented to Mr. Vanuxem, who examined it, having always been very sceptical on the subject of the fusion of charcoal. It consisted of a large and small globule connected together by a thread, colour black, without lustre, opaque. When struck it yielded without breaking, receiving a polish like that of iron; when filed it gave way as iron or soft steel would do; it was attracted by the magnet, and when hammered was malleable. Nitric acid, when heated, acted violently on it, and, ultimately, peroxide of iron and a little silica were obtained. The proportion of silex to metallic iron was about 11 : 5.

Such, therefore, is the nature of the black fused charcoal, and there can be no doubt that the colourless fused charcoal is also due to the impurities of the charcoal acted upon, as was formerly supposed *.—*Philadelphia Journal.*

Messrs. Silliman and Hare deny, however, that Mr. Vanuxem has operated on a proper specimen.

7. *Selenium in Anglesea Pyrites.*—An account is given in the *Annals of Philosophy, New Series*, ix. 52, of the production of selenium, during a process in which sulphuric acid, made from Anglesea pyrites, is used. The acid is prepared by Mr. Mutrie of Manchester, and is used by Mr. Thomson, who observed the production of the selenium in the preparation of muriatic acid. The selenium distils over with the muriatic acid into receivers, and in the course of two or three days, falls down as a reddish brown substance. The proportionate quantity produced from the sulphuric acid appears to be very small.

This selenium has been examined by Mr. Children: a fragment,

* Quarterly Journal of Science, xvi. p. 157.

heated on platina foil by a spirit-lamp, tinged the flame of an azure blue colour. A portion heated in a glass tube, gave off at first acidulous water, then some sulphur, afterwards a yellow vapour, condensing into a red sublimate, arose, and the residuum, by being heated in a tube open at both ends, left a grey earthy substance, principally silica and lime, amounting to about 53 per cent. of the weight of the original substance; consequently, there was about 47 of volatile matter, of which by far the largest portion was the red sublimate. It had fused and spread over the inner surface of the tube. A portion of it gave the same blue colour to flame before-mentioned, but more intense. Another portion, in an open tube, sublimed without giving off any sulphur, exhaling a strong odour like that of horseradish. It fused when heated gently, remaining awhile in a pasty state. It has a metallic lustre, a deep brown colour by reflected light; conchoidal fracture with a vitreous lustre; easily scratched by a knife; brittle; powder deep red; adhering when rubbed in a mortar, having then a grey, smooth, and somewhat metallic lustre. When in very thin lamina it is transparent, being of a beautiful cinnabar red colour.

8. *Alloy of Antimony and Potassium, first produced by Geoffroy.*—M. Serullas observed, amongst numerous other facts, on the production of alloys of antimony and potassium, that if emetic tartar be heated to whiteness in a covered crucible for two or three hours, there will be obtained, when cold, a carbonaceous mass which inflames spontaneously on exposure to the atmosphere. M. Serullas states, that this pyrophoric property of antimony, heated with carbon and potash, was pointed out by Klaproth, who, without knowing the cause, had observed the effect; but M. Derheins has more lately shewn, that the same property had been observed by Geoffroy, in 1736, and described by him at length, in *l'Histoire de l'Academie, &c.*, for that year. He calls it a new detonating phosphorus made with antimony. It was obtained by mixing one ounce of diaphoritic antimony with two ounces of black soap, putting the mixture by degrees into a hot crucible, ultimately adding another ounce of soap, covering up the vessel and heating it violently, it was left to cool. In the evening when opened, it suddenly took fire by contact of air, and burnt with explosion; there were no fluid scoria, but the whole had formed a spongy mass. The process was repeated several times and always with the same result.—*Journ. de Phar.* 1824. p. 631.

9. *Composition of Crystals of Sulphate of Soda.*—It is known that when a hot strong solution of sulphate of soda is put into a vessel and closed up, it may be reduced to common temperatures without crystallizing, although, if the vessel be opened, abundance of crystals will immediately form. It has also frequently been

observed that, in some circumstances, crystals would form in the solution during cooling, even though the vessel had not been opened or agitated. These crystals, when observed in the solution, are very transparent and of a large size; they are quadrangular prisms, with diedral summits. Upon opening the vessel, the surrounding solution crystallizes rapidly, enveloping the first formed set of crystals with others, which, however, are very readily distinguished from them in consequence of their immediately assuming a white opaque appearance. Upon taking out the crystals, those first formed are found to be much harder than the usual crystals of sulphate of soda, and, when broken, it is found that the opacity is not merely superficial, but that it penetrates them to a considerable depth, and even at times throughout.

These harder and peculiar crystals are readily obtained by closing up a solution of sulphate of soda, saturated at 180°, in a Florence flask, boiling the solution in the flask so as to expel the air before closing it. Upon standing 24 hours, fine groups of crystals are formed. When the flask is opened the solution deposits fresh crystals, but on breaking the flask, the latter may be scraped off by a knife in consequence of the superior hardness of the first set.

The hard crystals when separated are found to be efflorescent, like those of the usual kind, and they ultimately give off all their water, leaving only dry sulphate of soda. When a given weight was heated in a platina crucible, one half their weight passed off as water, the rest being dry salt. They, consequently, contain eight proportionals of water, or 72 sulphate of soda, and $8 \times 9 = 72$ water. The usual crystals of sulphate of soda contain 10 proportionals of water.

When crystallized sulphate of soda is heated in a flask, a part of it dissolves in the water present, whilst the rest is thrown down in an anhydrous state. The solution at 180° appears to contain one proportional of salt 72, and 18 proportionals of water 162; from which, if correct, it would result, that when the crystals are heated to 180° $\frac{5}{9}$ of the salt take all the water, whilst $\frac{4}{9}$ separate in the dry state.—M. F.

10. *Use of Chloride of Calcium, as a Manure.*—M. Chevalier finds that chloride of calcium is useful as a manure, only in the state of very diluted solution, for that, when applied in the solid state to the soil, it destroyed vegetation. It is something, however, to obtain a confirmation of the results mentioned, vol. xvii. p. 362. even though that confirmation be rather general.—*Jour. de Pharmacie*, 1824, p. 611.

11. *Separation of Strontia and Baryta.*—*Fluate of silica and baryta* precipitates in crystals almost insoluble. The *fluate of silica*

and strontia is very soluble in excess of acid. On this difference in properties, M. Berzelius founds a process for the separation of these two earths: he considers it an easy one and sufficiently exact for the estimation of their quantity. The mixture of the two earths is to be dissolved in muriatic acid, then solution of silicated fluoric acid is to be poured in, the baryta will precipitate, and its weight is determined by that of the precipitate; a very minute quantity of sulphuric acid added to the solution, will precipitate the rest of the baryta without acting on the strontia. The liquid is to be filtered: evaporated to dryness, and the residue decomposed by sulphuric acid.—*Ann. de Chimie.* xxvii. 301.

12. *On Combinations of Carbon and Iron, Pig Iron,* &c.—A long experimental memoir by M. Karsten, on the combination of iron and carbon, is contained in the Annales des Mines, ix. 657. We have not time at present to analyze it, but give the results as drawn up by the author, and appended to the memoir.

1. White cast iron and tempered steel contains the carbon combined with the whole mass of iron. 2. Lamellated white cast iron presents a perfect combination of iron with carbon; it always contains more carbon than the grey cast iron. 3. Iron and steel, not tempered, contain the carbon in the state of carburet. 4. Cold grey cast iron contains the larger part of its carbon, in the state of graphite and of mixture: this graphite contains no iron, but constitutes the carbon in all its purity. 5. The rest of the carbon contained in the grey cast iron, may be found either combined with the whole mass, or forming a definite carburet, which is afterwards dissolved in the metal, as is the case with soft iron, or steel. 6. All the varieties of carburetted iron, considered in the liquid state, contain the carbon dissolved in the mass of metal in indefinite proportions. 7. Finally. The graphite separates from the metal at the moment of congelation, and if there be other carburets of iron, they separate at a later period.

Remarking on the means generally proposed for the separation or estimation of the carbon in carburetted iron, M. Karsten finds grounds of objection to them all. That proposed by M. Vauquelin, namely, the use of sulphurous acid, he states to be uncertain and inaccurate from the formation of sulphuret of iron.

13. *Massive Copper obtained by the Moist Process.*—M. Clement has described the production of metallic copper from a solution, in a state as dense and compact as that afforded by fusion. It occurs in the manufactory of M. Mollerat during a process, the object of which, is to prepare sulphate of copper by the calcination of copper with sulphur. A solution of the sulphate is obtained turbid from the presence of insoluble sub-sulphate. This solution is introduced into a wooden tank, that it may become clear by deposition;

the tank is sank partly into the ground, and it is upon its internal surface, and always where two staves meet, that the mushrooms of metallic copper form; they appear as small masses, enlarge by degrees, and ultimately are of considerable size; one weighed 75 grammes (1158 grains).

When detached from the wood, they are found on one side to be marked by the stries of the wood, whilst the other surface is mamillated, and present very small, but brilliant crystalline facets. They are formed, in consequence of the presence of sulphate of protoxide of copper, in solution, which, in passing to the state of per-sulphate, deposits one portion of copper in the metallic state, whilst the other is peroxidized. The effect does not require access of air. M. Clement was particularly struck with the cohesion acquired by the copper thus precipitated from a solution; it was such as to allow of the metal being forged at common temperatures, and being reduced to thin leaves. Its specific gravity, also, was 8.78 being equal to that of fused copper. When filed, the surface produced was as brilliant and as close as that of an ingot of common copper.—*Ann. de Chim.* xxvii. 440.

At some of the Anglesea copper mines, the solution of sulphate of copper pumped up by the engines is decomposed by the introduction of iron, and the copper is precipitated. It frequently happens there, that the circumstances are such as to produce copper as compact and dense as fused copper, and there is no difficulty in selecting such specimens from the pits in which the precipitation is usually performed.—Ed.

14. *Ammoniacal Chromate of Copper.*—M. Vuaflart has observed, that chromate of copper prepared by precipitating sulphate of copper by chromate of potash, and which is of a reddish brown colour, is soluble in diluted ammonia, producing a clear solution of a beautiful and deep green colour. When the solution is evaporated, the reddish chromate of copper appears as the ammonia flies off.

This solution was made for the purpose of decorating the front of a druggist's shop. The green is finer than most of those obtained in the usual manner, and undergoes no change by length of time or exposure to bright light. It is readily prepared by adding solution of chromate of potash to ammoniacal sulphate of copper.—*Jour. de Phar.* 1824. p. 607.

15. *Artificial Crystals of Chromate of Lead.*—A diluted solution of nitrate of lead being added to a very alkaline solution of chromate of potash, and left at rest for some time; there was found in the mixed solution small red crystals, which, upon examination, proved to resemble, in all their characters, the native chromate of lead from Siberia.—M. F.

16. *Compound of Muriate and Hydrosulphuretted Oxide of Antimony.*—Sulphuretted hydrogen added to solution of muriate of antimony, throws down a yellow coloured precipitate, generally regarded as a hydrosulphuretted oxide of antimony, but which is a compound of that substance with neutral muriate of antimony. Heat separates the latter body, and sulphuret of antimony remains. A similar effect is produced by exposing the precipitate in a close vessel to the solar light.—*Gmelin. Ann. Phil. N. S.*

17. *New Mineral. Titaniferous Cerite.*—M. Laugier has lately analyzed a mineral from the Coromandel coast, which, from its composition appears to be peculiar and distinct. It was brought to Europe by M. Leschenault de la Tour. It was an irregular mass, of a blackish-brown colour, a vitreous conchoidal fracture, hardness equal to that of the Gadolinite, to which mineral it had some analogy, but differing from it by swelling up when heated. It lost only 1.25 per cent when heated, although it contained $\frac{1}{10}$ of water; a cause for this effect will be evident presently.

Acids and alkalies both act upon it, and M. Laugier employed these agents in his analytical experiments. He found it to yield 36 oxide of cerium; 19 oxide of iron; 8 lime; 6 alumine; 11 water; 1.8 oxide of manganese; 19 silica; 8 oxide of titanium. These quantities surpass the 100 used by 9.55 parts; this is occasioned by the protoxide of cerium which exists in the mineral becoming peroxide during the process, and this is also given as the reason why so little loss of weight occurs when the substance is calcined.

M. Laugier remarks, that this mineral is analogous in its composition to the substances distinguished by Berzelius and Hissinger as Orthite, Allanite, and Cerine; that it particularly resembles *orthite*, especially in its physical characters, but differs in the presence of titanium. It may therefore be regarded as a new variety of *titaniferous cerite.*—*Ann. de Chimie.* xxvii. 313.

18. *On Chloride of Titanium.*—Having the opportunity of examining the foundation of a blown out furnace, at the Low Moor Iron Works, Mr. E. S. George removed from it a quantity of the stone work, penetrated completely by metallic iron, sulphuret of iron, carbonaceous matter, and titanium in brilliant cubes. Part of this mixture, acted upon by muriatic acid, evolved hydrogen, and sulphuretted hydrogen, the iron and earths were dissolved, and a mixture of titanium cubes and grains of sand was left; the silex was easily removed.

Sixty grains of the metallic titanium were placed in a glass tube and dry chlorine passed over; no action was perceptible until the titanium was heated to redness, but then a fluid gradually condensed in the cool part of the tube, and was collected by in-

clining the apparatus. The fluid was transparent, colourless, possessing considerable density, evolving dense fumes in the atmosphere, having a pungent odour resembling that of chlorine: the fumes depend upon the presence of moisture: it boils a little above 212°, and condenses without further change. A drop of water added to a few drops of the liquid caused rapid disengagement of chlorine, much heat, and the production of a solid salt. This salt is deliquescent, soluble in water, the solution having all the properties of muriate of titanium. It yields a brownish-red precipitate with prussiate of potash, a dark red with infusion of galls, with pure potash a gelatinous precipitate, soluble in excess of muriatic acid; ammonia throws down a white precipitate. When the chlorine is not dry, the same salt crystallizes in the tube. Into a long test tube, 14.6 grains of the fluid were introduced, and afterwards a weighed portion of water added very gradually; chlorine was rapidly disengaged, and heat produced. When cold, the loss was found to be four grains, and the solution gave a dark-red precipitate with gallic acid. The fluid, therefore, is a perchloride of titanium, which, by losing chlorine, becomes a protochloride, and that by solution in water a muriate.

Water was added to a solution of the muriate formed by the decomposition of the perchloride by water, and the solution divided into two equal parts: the one decomposed by potash gave 7 grains oxide of titanium; the other, by nitrate of silver, gave 15 grains chloride of silver=3.6 chlorine. The muriate, therefore, contains oxide of titanium, 7; muriatic acid, 3.74. Supposing the muriate to contain one atom acid, and one atom oxide, the latter will be the protoxide, and the weight of titanium will be 61.2. Probably, the true number is 64, as indicated by Mr. Rose's experiments.

Muriate.	Oxide of Titanium . .	7.00
	Muriatic acid . . .	3.74
Protochloride.	Titanium	6.12
	Chlorine	3.64
Perchloride.	Titanium	6.66
	Chlorine	7.94

Ann. Phil. N. S. ix. 18.

19. *Peschier on Titanium in Mica.*—In consequence of the results obtained and published by M. Vauquelin*, with regard to the presence and the quantity of titanium in various specimens of mica, M. Peschier, to whose results those of M. Vauquelin's are opposed, has published the process by which he separates

* Quarterly Journal of Science, vol. xviii. p. 392.

titanium from other substances, in minerals containing it, and which he thinks requisite to its correct estimation.

1. The mineral is to be finely pulverised, heated with two parts of potash, the crucible being taken from the fire when incandescent; the produce is to be diffused through water, put on to a filter, and washed, until test paper ceases to be affected. The washings are slightly supersaturated, evaporated to a moist solid state, diffused through water, and put on to a filter: the silica separated when washed and dried, is treated with oxalic or muriatic acid, and the washings added to the former washings; infusion of galls is then added, and the solution made slightly alkaline and concentrated; if the characteristic brown red tint of titanium appears, the solution is set aside, to be subjected to further examination.

2. The residue, insoluble in potash, is boiled with a mixture of one muriatic acid and six or eight of water; if a larger quantity of insoluble matter than was expected appears, it is treated with potash, as before. The acid solutions are saturated with an alkaline subcarbonate, and after having separated the precipitate, the liquid is examined, as was that in section one, and if it contains titanium, is mixed with it.

3. The precipitate formed in the last section, is exposed to the action of potassa, and as the titanium is partly dissolved in the alkali with the alumina, and partly thrown down again with it, by muriate of ammonia, sulphate of ammonia is used instead, which is found to precipitate only alumina; when this earth has been received and washed on a filter, the liquids are evaporated to a moist saline state, and by solution in water, such portion of silica separated from it as was previously dissolved. Infusion of galls is added to the washings, all of which are added to the preceding washings of 1 and 2.

4. As the titanium does not dissolve in potash so readily as the alumina which accompanies it, the insoluble portion preserves a gelatinous appearance; to separate the titanium, the residue is dissolved in muriatic acid, the silica removed by a filter, the iron separated by ferroprussiate of potash, the liquid saturated by an alkaline subcarbonate, and boiled. The precipitate which forms, is white, bulky, and resembles alumina in appearance; it may be composed of oxide of titanium, magnesia, and lime; a strong heat renders the first insoluble in acids, and consequently, the earths may be removed by digestion of the whole for some hours in weak acid, as distilled vinegar. The insoluble portions are to be separated on a filter, the liquid treated with ammonia for the magnesia, and with oxalate of ammonia for the lime; the operation has been well performed, if infusion of galls produces no effect in the remaining solution.

5. As titanium forms double salts with all the acids, and as its

tannate is readily dissolved by infusion of galls, the portion of titanium which otherwise would be lost, from the influence of these two causes, may be obtained by evaporating to dryness all the solutions which have been set aside, heating the residue to redness, dissolving in water, filtering, washing the insoluble portion, again heating it, to burn off carbonaceous matter, then washing it with weak acid, and it will be the titanium required. If contaminated by iron or manganese, they may be removed by digestion in nitromuriatic acid, and repeating this process twice more on the washings, adding each time infusion of galls, all the titanum may be obtained from the mineral analyzed.

In consequence of the tendency of titanium to form double salts, I have always separated several grains from the salts obtained in the search after the alkaline principle of this class of minerals; its presence is known by the spongy state of the chlorides of potassium or sodium, which, when freed from ammonia, and heated to redness, will not enter into fusion, and which require several solutions, evaporations, and calcinations, for its complete separation.

Such are the minute operations which M. Peschier has found indispensably necessary in the analysis of minerals with a base of titanium (the number of which is greater than is supposed), and by the aid of which, the foliated black mica of Siberia gave—Silica, 24; Alumina, 8.5; Magnesia, 5; Peroxide of iron, 30; Manganese, 0.7; Titanium, 21; Potash, 5.7; loss by fire, 2.75; total 97.65. Talcs, Chlorites, and Steatites, have yielded from 19 to 30 per cent. of a substance which, like that from mica, which M. Peschier has called titanium, and the titanum obtained from rutilite, forms a gelatinous transparent yellowish mass by evaporation at a gentle heat, from its muriatic solution; furnishes, like it, a very voluminous gelatinous and white precipitate, by the saturation of its acid solution; which yields a yellowish precipitate by infusion of galls, deepening in colour by a slight supersaturation of the acid, becoming brown, and dissolving by further addition of the re-agent, producing a blood-red solution; is soluble in pure alkali; forms double salts with all the acids; becomes insoluble in acids by a strong heat, and consequently, possesses all the characters of titanium, with this slight difference, that its precipitate by infusion of galls is not quite so abundant or so deep in colour, nor does it always become of a brown colour by heat; these differences are, however, considered as of but little importance, compared with the many positive characters it possesses.—*Ann. de Chimie*. xxvii. 281.

20. *Wöhler on a Compound of Cyanuret and nitrate of Silver.*—Concentrated solutions of cyanuret of mercury and nitrate of silver, being mixed together, no precipitate fell, but after a few minutes,

small white crystals formed, which were repeatedly washed with water, and dried. These, heated above 212°, first fuse, then boil up, and detonate vehemently with a cracking noise, and a purplish flame, resembling that of cyanogen. The residue is cyanuret of silver, which, by continued ignition in the air, becomes metallic silver. In close vessels, mercury sublimes during the experiment. Muriatic acid disengages hydrocyanic acid from the crystals which, driven off by heat, is succeeded by strong odour of chlorine; on evaporation chlorides of silver and mercury remain. Solution of the crystals mixed with muriate of baryta, filtered and evaporated, yields a saline mass, containing octoedral crystals of nitrate of baryta. Alcohol also extracts from it cyanuret of mercury; consequently, the original crystals are a compound of cyanuret of mercury and nitrate of silver.

This substance is difficultly soluble in cold, but plentifully in hot water; it crystallizes in large transparent prisms, like those of nitre. Alcohol dissolves it as much as water does. It is soluble in boiling hot nitric acid, without decomposition. Alkalies precipitate cyanuret of silver from its solution, mixed also with sub-nitrate of mercury. Repeated solution in pure water effects a similar decomposition, to a slight extent. Heated below 212° they lose water, and become opaque, without altering in form; 100 parts gave off 7.6 of water.

To determine the quantity of silver in the compound, one gramme was treated with excess of muriatic acid, carefully evaporated to dryness, and the corrosive sublimate expelled by heat, 0.32 of a gramme of fused chloride of silver remained, equal to 0.2588 of oxide of silver, or 37.96 per cent. of nitrate of silver. The cyanuret of mercury was ascertained by dissolving 0.67 of the crystals in hot water, precipitating the crystals by (hydro?) cyanic acid, filtering, and evaporating to dryness, to expel the excess of acid and the nitric acid, 0.36 of pure cyanuret of mercury remained = 53.74 per cent. Hence, 100 parts consist of

Nitrate of silver . . .	37.96	— 1 atom
Cyanuret of mercury . .	53.74	— 2 ——
Water	7.60	— 3 ——

This compound must be regarded as a true saline substance, in which nitrate of silver acts as acid, and cyanuret of mercury as base; and the existence of water in it, a substance possessed by neither of its elements in a separate state, affords additional reason for ranking it among salts.

M. Wöhler then endeavoured to form other similar compounds of nitrate of silver with metallic cyanurets. Newly precipitated cyanuret of silver, boiled in solution of nitrate of silver, dissolved slowly but completely. As the temperature fell below the boiling point, there were deposited white acicular crystals in

such quantity, that the liquid became a magma. These were dried upon bibulous paper; they would not bear washing, the affinity by which the compound was formed being so weak that contact with water resolved it into pulverulent cyanuret of silver, and solution of the nitrate, so that strong solution of nitrate of silver is required in its preparation. When heated it fuses, detonates, and leaves cyanuret of silver; it contains no water. If analogous to the preceding salt, it should contain

Nitrate of Silver . . .	38.79	—	1 atom
Cyanuret of Silver . .	61.21	—	2 ——
	100		

Equal to 70.76 per cent metallic silver. This statement was confirmed by the results of an experiment; 0.43 of the compound decomposed by muriatic acid giving 0.387 chloride of silver, or 69.74 per cent. metallic silver.

All attempts to form other similar compounds failed, the other metallic cyanurets decomposing the nitrate of silver used in the experiment.—*Ann. Phil. N. S.* ix. 131.

21. *Sulphates of Cinchonia and Quinia.*—The following results and remarks are abstracted from a memoir by M. Baup.—*Ann. de Chimie*, xxvii. 323.

Super Sulphate of Cinchonia.—Prepared by adding very pure sulphuric acid to the neutral sulphate, evaporating until a pellicle forms, and setting it aside to crystallize. If the crystals are small and fragile they should be re-crystallized; if not soluble in their weight of cold water, it is a proof that neutral sulphate is present, when more acid must be added. This salt is colourless, unaltered at common temperatures in the air, but readily efflorescing if the temperature be slightly raised; crystals are rhomboidal octoedra, always imperfect, and readily cleaving perpendicularly to the larger axis. It is soluble in 0.46 of water at 57°.2, or in 0.9 of alcohol, specific gravity 0.85. Ether does not dissolve it.

Neutral Sulphate of Cinchonia.—Crystals short rhomboidal prisms of 83° and 97° cleaving parallel to the planes of the prism, soluble in 6.5 of alcohol, specific gravity 0.85 at 55°.4, or in 11.5 of absolute alcohol, or in 54 of water.

The elements of these salts were estimated as follows:—The water from the loss occasioned by exposure in a stove, at a temperature of 248° Fahr. until no further diminution took place; the sulphuric acid by precipitation from a solution of the sulphate in diluted acetic acid, by muriate of baryta; the cinchonia by the deficiency. From these experiments it was deduced that 39 would

represent the number of cinchonia, oxygen being 1 *, and that the neutral sulphate contained

			Crystallized	Dry
1 atom	cinchonia .	39	84.324	88.636
1 ——	sulphuric acid	5	10.811	11.364
2 ——	water . . .	2.25	4.865	100.000
			100.000	

Super Sulphate of Cinchonia.

			Crystallized	Dry
1 atom	cinchonia .	39	67.241	79.592
2 ——	sulphuric acid	10	17.241	20.408
8 ——	water . . .	9	15.518	100.000
			100.000	

Neutral Sulphate of Quinia.—This salt readily effloresces in the air, three-fourths of the water passing off, and one-fourth remaining. Soluble in 740 of water at 55.4° Fahr., and in 80 parts at 212°. Alcohol of specific gravity 0.85 dissolves $\frac{1}{60}$ at common temperature ; in much greater proportion at the boiling heat.

Super Sulphate of Quinia.—When pure, colourless, unaltered in the air at common temperatures: crystallizes in rectangular prisms, cleaving parallel to the planes of the prism. The crystals obtained by diminished temperature are small and acicular, but spontaneous evaporation yields them of a large size. It is soluble in 11 of water, at 5°.4 Fahr.; in 8 parts, at 71°.6 Fah.; and in its water of crystallization at 212° Fahr. It is soluble in alcohol, and the crystals obtained from solution in absolute alcohol immediately fall into powder in the air.

Some of the crystals still wet were put upon filtering paper, in a cold damp place: when dry they were rapidly weighed, and then left to effloresce at a temperature of 68°. In this way it lost three-fourths of its water, and the other fourth also when dried in a stove. The dry sulphate exposed to the air resumed the fourth it had lost in the stove.

The experiments of M. Baup led him to adopt the number 45 for quinia. The composition of its sulphates are according to him as follows :

Super Sulphate			Crystallized	Dry
1 atom	quinia . .	45	61.644	81.819
2 ——	sulphuric acid	10	13.698	18.181
16 ——	water . .	18	24.658	100.000
			100.000	

Neutral Sulphate			Crystallized	Dry
1 atom	quinia . . .	45	76.272	90
1 ——	sulphuric acid	5	8.474	10
8 ——	water . .	9	15.254	100
			100.000	

* Cinchonia 312, hydrogen being 1. Quinia, 360.

Neutral Sulphate effloresced

1 atom quinia	. . . . . .	45	. . . . 86.12
1 —— sulphuric acid	. . .	5	. . . . 9.57
2 —— water	. . . .	2.25	. . 4.31
			100.00

M. Baup also remarks that in medicine the same doses of neutral sulphate of quinia, which after having been merely dried in a cool dry place is preserved in a well-closed bottle, or of the same sulphate kept in a badly-closed bottle or in the air, should in no case be employed indifferently. In the first case the salt may contain only 76 per cent of quinia, whilst in the latter it may rise as high as 86 per cent. He considers, that constancy would be most readily ensured by the use of the effloresced salt, as it is always of definite and invariable composition.—*Ann. de Chim.*, xxvii. 323.

22. *Pariline, or salifiable Base of Sarsaparilla.*—A new vegeto-alkaline base is said to have been discovered by M. Galileo-Palotta, in sarsaparilla. The process for obtaining it is as follows: The sarsaparilla is to be cut, crushed in a mortar, six times its weight of common boiling water poured on to it, and the infusion to be continued for eight hours in a closed vessel; the liquor is then to be passed through a cloth, an equal quantity of water poured on to the residue, and treated as the first portion. The infusions added together have a deep amber-colour, and are slightly bitter and nauseous. Sufficient milk of lime is to be added, until the mixture sensibly reddens turmeric paper, agitating the liquid strongly at the same time with a wooden spatula. The infusion changes colour and becomes brown, and ultimately a grey pulverulent substance precipitates, which is to be collected on a fine cloth, and whilst moist mixed with water saturated with carbonic acid; it is then to be dried in the sun, and reduced to a fine powder. This powder is to be boiled in a matrass for two hours with alcohol, specific gravity .817; filtered, and the residue redigested in fresh alcohol.

The alcoholic solutions being mixed, are to be distilled in a glass retort by a water-bath, until the liquid in the retort becomes sensibly turbid, when it is to be put into a capsule and left at rest: in a short time a white pulverulent substance precipitates, and attaches itself to the surface of the vessel. The liquid is to be poured off, the vessel put into a stove heated to 25° R (88° Fahr.) When sufficiently dry, it is be collected and preserved in close vessels, being *Pariline*. The liquid decanted and evaporated slowly to dryness, yields a solid compact mass, slightly deliquescent, and of a dull colour. It is pariline combined with a particular colouring matter, but may be purified readily by the usual means.

Parilline is white, pulverulent, light, unaltered in the air, bitter, austere, slightly astringent and nauseous, and having a particular odour. It is heavier than distilled water. Insoluble in cold water; very little soluble in hot water. Slightly soluble in cold alcohol; soluble in hot alcohol. Impure parilline is insoluble in cold water, soluble in hot water, and in concentrated hot and cold alcohol.

This substance slightly reddens turmeric paper. Heated to 212° it fuses, becomes black, is partly decomposed: at a higher temperature it decomposes like vegetable substances containing no nitrogen. Concentrated sulphuric acid decomposes it: diluted sulphuric acid is neutralized by it, forming a sulphate. All the acids unite to it, forming salts.

After certain experiments made upon himself, in which M. Palotta took at different times: from 2 to 13 grains of pure parilline at once, he concludes that the substance is a powerful debilitating medicine, or one generally diminishing the vital energy; and, that in proportion to the dose given: to this property it also unites irritating powers.

M. Planche states that he has repeated the process of M. Palotta, but has not as yet verified all the properties said to belong to the substance.—*Jour. de Phar.* 1824, p. 543.

23. *Experiments on Civet.* By M. Boutron-Charlard.—The experiments of which the following are an abstract, were made with an unexceptionably good and unaltered specimen of civet. It was a semi-fluid mass, unctuous, of a yellowish colour, becoming brown by time, thickening by contact with air, of a very strong and disagreeable odour when in quantity, but agreeable when weakened. A portion of it put into a close vessel in which also was placed a piece of reddened litmus paper, gave out ammonia enough in 24 hours to restore the blue colour of the paper: when distilled at a low temperature, a few drops of an ammoniacal solution came over. Digested with boiling ether a portion was dissolved, which, when obtained by evaporation, proved to be a mixture of elaine and stearine. The part insoluble in ether was soluble in hot solution of potash, with the exception of a few hairs and extraneous matter: the addition of nitric acid precipitated flocculi, of a substance which when collected burnt like animal matter. Hot alcohol after some time entirely dissolved the pure part of civet; on cooling, stearine was deposited, the remaining solution became turbid when dropped into water; evaporated it left an orange-yellow semi-fluid substance, which, by diluted muriatic acid and heat, was separated into resin and fatty matter.

Civet distilled with water yielded a few drops of volatile oil, of a biting hot taste, and having the odour of civet. The residue

gave an aqueous solution which evaporated to dryness, and freed from resin and extract by alcohol, left a peculiar yellow colouring matter, soluble in water, and combining with various bases. When incinerated, civet gave a voluminous coal, alkaline, and containing carbonate and sulphate of potash, phosphate of lime, and oxide of iron. Hence civet appears to contain, free ammonia, stearine, elaine, mucus, resin, volatile oil, yellow colouring substance, salts, *&c.* No benzoic acid could be found in it.—*Jour. de Phar.* 1824, p. 537.

24. *Composition of Common and Deoxidized Indigo.*—Mr. Dalton, in a paper published in the *Manchester Memoirs*, states, that in various experiments made at different times, but with very similar results, he finds that the quantity of oxygen required to convert the green or deoxidized indigo solutions in lime water into blue indigo is about $\frac{1}{7}$ or $\frac{1}{3}$ of the weight of the resulting indigo; and on the supposition that an atom of oxygen was added to one of indigo, concluded that the atom of indigo must weigh about 55 or 56. When indigo is destroyed by the oxymuriate, or rather, chloride of lime, as in the process adopted by Mr. Dalton for testing the value of indigo, he is persuaded from his experiments, that twice the quantity of oxygen is necessary that is required to revive it from the lime solution.

25. *Decolouring power of different substances.*—From certain experiments made by M. Payen on the decolouring power of various carbonized, mineral and other substances, it results that the schists of Mermat in Auvergne, and the carbon of calcined bones decolour in the highest degree. The schist removed $\frac{3}{5}$ of the colouring matter, the carbon $\frac{4}{5}$.—*Ann. de Chim.* xxvii. 360.

26. *Prize questions.*—By the Royal Academy ot Sciences of Toulouse, for 1826, "A physico-mathematical theory of lifting and forcing pumps, demonstrating the ratio between the moving power employed and the quantity of water raised, the height to which it is raised being known, and all the obstructions which the force would have to overcome being considered." The prize a gold medal of 1000 francs value.

For 1827 "The manner in which the known anti-fermentative and anti-putrescent re-agents, such as camphor, garlic, peroxide, and perchloride of mercury, sulphurous acid gas, *&c.*, oppose the spontaneous decomposition of vegetable and animal substances, and thus prevent the formation of alcohol in the former, and developement of ammonia in the latter." A medal 500 francs in value.

III. Natural History, &c.

1. *Antiquity of Trees.*—In "Major Rooke's Sketch of the Forest of Sherwood" it is stated, that in cutting down some timber in Berkland and Bilhaugh, *letters* have been found cut or stamped in the body of the trees, denoting the king's reign in which they were marked. It seems that the bark was cut off, and the letters cut in, after which the next year's wood grew over it, but without adhering where the bark had been cut. The ciphers are of James I., of William and Mary, and one of King John! One of those with James's cipher was about one foot within the tree, and one foot from the centre: it was cut down in 1786. The tree must have been two feet in diameter, or two yards in circumference, when the mark was cut. A tree of this size is generally estimated at 120 years growth, which number subtracted from the middle year of James's reign, would make 1492 the date of the planting of the tree. The tree with William and Mary had the mark about nine inches within the tree, and three feet three inches from the centre; cut down also in 1786. The mark of John was eighteen inches within the tree, and something more than a foot from the centre: it was cut down in 1791; but the middle year of John's reign was 1207, from which, if we subtract 120, the number of years requisite for a tree of two feet diameter to arrive at that growth, it will make the date of its planting 1085, or about twenty years after the conquest. The tree, therefore, when cut down in 1791, must have been 706 years old; a fact scarcely credible; for it appears from the trees whose marks are better authenticated, that those exactly of the same size, when marked, had increased twelve inches in diameter in 173 years, whilst this tree had increased no more than eighteen inches in 584 years. Major Rooke says that several trees with this mark had been cut down, so that deception or mistake is scarcely possible.—*N. M. Mag.*

2. *Red Snow of the Alps.*—The notice, of which the following is part, was read to the Society of Natural History of Geneva, by M. Peschier: "I received in September, from M. Barras, Canon of the Convent of St. Bernard, a small bottle of water collected from the melting of this snow. The note accompanying it stated, that the spots of red snow assumed a deeper tint as the season advanced; that the portion from whence the water was procured had a coffee colour on its surface, but on removing two inches in depth, had a red colour. A deposit of the colour of moist earth occupied the bottom of the bottle, but on inclining it, it was found that the deposit reflected a red tint, like that of the snow; and having, in company with MM. de Candolle and Prevost, examined

it microscopically, we found that the red tint was due to the presence of small spherical globules of a bright red colour, which were surrounded by a gelatinous membrane, transparent, slightly yellow, the size of which varied from three to six millimetres in apparent diameter: in certain cases they were arranged in lines representing fibres; and they were mixed with fragments of moss and dust detached from the rocks.

A comparative observation was made on the deposit from the water of the red snow of the north, brought by Captain Ross, of which M. de Candolle possessed a small quantity; and it was found that the globules in it were identical with those of the Alp snow; so that these spots must be due to the developement of this kind of plant. M. de Candolle, who has studied them closely, does not view them as belonging to the *Uredo*, but rather as forming a new genus.—*Bib. Univ.* xxvii. 132.

3. *Artificial Production of Pearls.*—Mr. Gray whilst examining a specimen of the shell *Barbala Plicata*, in the British Museum, observed on it several very fine regular shaped semi-orbicular pearls of most beautiful water, and afterwards on examining the collection of pearls at the same place, he had an opportunity of observing in one of them attached to a fragment of the same shell, and cracked across, that it was formed of a thick coat consisting of several concentric plates formed over a piece of mother-of-pearl roughly filed into a plano-convex form like the top of a mother-of-pearl button: the other pearls all appeared to be formed in the same manner, and on some pieces of shell where the pearl had been destroyed or cut out, there was left a circular cavity with a flat base about the depth, or rather less, than the thickness of the coat which covered the pearls; proving that the pieces of mother-of-pearl had been introduced when the shell was younger and thinner, and that they must have been introduced between the leaf of the mantle and the internal coat of the shell.

Hence Mr. Gray was induced to expect that pearls of a very beautiful appearance and form for setting might be obtained with facility at home. He introduced similar pieces of mother-of-pearl into the shell of the *Anodonta Cygneus* and *Unio Pictorum;* this was done without much difficulty, the valves of the shell being forced open to a moderate breadth, retained so by a stop, the mantle slightly turned down, and the pieces introduced to some little distance by a stick; the stop was then withdrawn, and the animals returned to their natural habitation: of the thirty or forty pieces introduced, only two were pushed out again, the rest being placed by the animal in a convenient situation. In several afterwards destroyed, they were found near the posterior slope of the shell, where the pearls are situated in the *barbala.*

This plan of forcing the production of pearls by fresh water

bivalves, Mr. Gray thinks is the invention of the Chinese. On cutting out the pearls, it would be necessary that the shell should be cut through, so that the mother-of-pearl button may be preserved in its place, for if the back were removed, as it would be, were not the shell cut through, the basis would fall out, and then the pearl would be brittle.—*Ann. Phil.* N. S. ix. 27.

4. *Permanency of Human Hair.*—M. Pictet has lately made a comparison between a recent human hair and those from the head of a mummy from the Isle of Teneriffe ; with respect to the constancy of those properties which render hair important as a hygrometric substance. For this purpose hygrometers, constructed according to Saussure's principles, were made, one with a recent hair, and the other with hair from the mummy. The ancient hairs were not so strong as the other, or of sufficient length alone, but the latter objection was obviated by tying four together. The results of the experiments were, that in both instruments, the interval between extremes of moisture and the dryness of the chamber (about 25°) was passed in three minutes: that the indications, like those of the thermometer, &c., were rapid on leaving the first term, and became slower on approaching the second: that the hygrometric quality of the Guanche hair is sensibly the same as that of the recent hair.—*Bib. Univ.* xxvii. 120.

5. *M. Peschier on the Cure of the Goitre.*—Being frequently called upon to administer aid in cases of goitre, M. Peschier, in 1816, endeavoured to separate from burnt sponge the substance which conferred on it useful properties; and thinking this might be the alkali, was induced to administer solution of sub-carbonate of soda, more or less disguised by other substances. This attempt was accompanied by complete success which has never yet been falsified. The effect was such, that M. Peschier says, at Aubonne (Canton de Vaud), and the neighbouring places, the remedy soon acquired a considerable name for its power of dispelling, or considerably diminishing, the goitre; and he refers to the evidence which may be abundantly obtained there, for confirmation. One or two cases are, however, quoted. Jan. 1. A young girl (Aubonne Isaline Cretigny), 14 years of age, well formed for her age, was brought to him for assistance. She had a goitre large enough to give the neck the appearance of a cylinder of the diameter of the head. Sub-carbonate of soda was administered in the proportion of two gros (118 grains) each day. At the end of the 20th day the goitre was so far diminished, and the girl's appearance so much altered, that she could scarcely be known for the same person. This was a particularly favourable case.

In ordinary cases, when the goitre is not connected with any general or constitutional affection, M. Peschier dissolves from two gros to half an ounce of sub-carbonate of soda in eight ounces

of water, and directs that a table spoonful of this solution should be taken twice a day in half a glass of wine, or sugar and water. Persons who have had no objection have taken the pure solution.

In cases where the enlargement of the thyroid gland has been accompanied by the same affection of the lymphatic ganglions of the neck, bitter and tonic roots, such as gentian, *&c.*, have been added to the alkali; and also purgatives administered, such as rhubarb and senna, with anise or fennel seeds, the whole infused in a bottle of good wine, of which a quarter of a glass has been taken two or three times a day.

In one case, among many others, relief was afforded to a young person who had several enlarged glands on each side of the neck, even after it had been proposed to extirpate them by a surgical operation, the remedy being continued for several months. In other cases, very old suppurations of the glands have been corrected and cured, after they had resisted various modes of treatment.

When, in 1820, Dr. Coindet proposed the use of iodine, M. Peschier also applied it, but, except in one instance, always with the solution of soda. In one case, where tincture of iodine alone was used, the disease resisted the medicine for six weeks, and, at the end of that time, had become hard, producing a sensation of strangulation. Leaving off the use of iodine, Dr. Peschier first gave purgatives, and then alkali, and attained the end required.

From that time M. Peschier says he has resumed the exclusive use of sub-carbonate of soda, and always with success, He suggests the propriety of observing, whether the inhabitants of those places where the water is slightly alkaline are not less liable to goitre than in other places; and whether mixing habitually a small quantity of soda in the water intended to be drunk, would not entirely prevent the occurrence of this disease in places where it is now most readily manifested.—*Bib. Univ.* xxvii. 146.

6. *On Digestion in Ruminating Animals, by* MM. Prevost and Royer.—The experiments of these philosophers were made on sheep, the stomach of ruminating animals offering many facilities in the examination of the phenomena of digestion in consequence of its division into four parts.

The masticated food, moistened with saliva in the mouth, passes by the œsophagus into the paunch (herbier), a large cavity, occupying the greater part of the left side of the abdomen; its internal surface is provided with papillæ, formed from the mamellated tunic, which are covered with an epidermis, readily separating into plates and shreds. The paunch communicates by a large aperture, with the second division placed to the right of the œsophagus: the mamellated tunic here presents folds which project considerably, and circumscribed polygons, the areas of which are covered with fine papillæ. The food in this division is less solid than that

in the paunch; returned to the mouth many times during rumination, it finally forms a paste, which passes directly from the œsophagus to the third stomach, by means of a groove, proceeding from the cardiac aperture of the paunch to the upper orifice of the third stomach.

The contents of the first and second divisions are similar to each other: the triturated mass which they contain is sensibly alkaline, probably because of the unsaturated soda of the saliva, and of the secretions of the two first stomachs. These were pressed together, and furnished a liquid and a hard residue. The liquid, boiled to separate the albumen, was then very carefully evaporated to dryness. The extract being put into warm water, left an insoluble portion of coagulated albumen, and by filtration a liquid was obtained, which, when evaporated, had on its surface a pellicle formed, which dissolved when stirred as jelly would have done. When cold the solution gelatinized, and upon being dried, left a brown substance, having a vitreous fracture, and slightly transparent. This substance had many characters of gelatine; it was insoluble in alcohol and ether, soluble in cold water, more soluble in hot water. Mineral acids or corrosive sublimate did not precipitate it when cold, but boiled with the latter, flocculi formed, insoluble, and the liquid lost its power of gelatinizing. The portion of residuum insoluble in water was coagulated albumen, containing a little mucus, which could be separated by acidulated water, and then obtained by evaporation. These experiments with others lead MM. Prevost and Royer to conclude that the nourishing parts of the alimentary matter are, 1. Albumen of the vegetables extracted and retained in solution by the alkaline fluids proper to the animal; and 2. Jelly, of which the properties have been mentioned, containing a certain quantity of mucus. The following result will give a general idea of the quantities concerned.

Alimentary portion of the two first stomachs	5.231	kil.
Liquid obtained by expression . .	2.753	
Residue of the expression . . .	2.478	
There was obtained from the liquid,		
Dried jelly . . .	16.78	grammes.
Dried albumen and mucus . .	27.52	
And from the residue of the expression,		
Dried jelly . . .	8.10	
Dried albumen and mucus . . .	4.82	

The albumen and jelly had been washed with alcohol, to free them from chlorophyle and salts.

The third stomach has its cavity filled by numerous folds of the mamellated membrane: these are thin, large, and placed one against another like the leaves of a book. These strongly compress the food; the liquid it contains is separated, and runs into

the fourth stomach, which like the last is on the right of the paunch. It is larger in size, and communicates at the lower part with the duodenum, by an aperture corresponding to the pylorus of single stomachs; a very delicate mucous membrane covers it internally, presenting large valves, disposed in a longitudinal direction. The liquids which come into it from the third stomach undergo a remarkable change, from alkaline becoming acid, and a white opalescent flocculent matter precipitates on the valves, to which it adheres as a false membrane would do. This precipitate is the chyme, it is nearly pure albumen, and contains globules; it does not dissolve either in cold or hot water, or in mineral acids or alcohol, but is very soluble in alkalies. The chyme and the parts of the food pressed in the third stomach are evacuated into the duodenum, and brought into contact with the alkaline secretions of the liver and pancreas. The chyme becomes an emulsion containing globules; the albumen remaining in the vegetable matter, is extracted during the passage through the intestines, whilst, by a particular set of vessels commencing on the surface of the latter, the nutritive portion called chyle is absorbed, conveyed to the thoracic duct, and from thence into the sanguiferous system. The chyle of the sheep and horse is white, opalescent, readily coagulating when received into vessels, the clot swimming in the serum, which ultimately separates: it becomes slightly reddened on exposure to air.

One ounce of chyle was obtained from a moderately large sheep: the coagulated portion washed, compressed in a cloth and dried, gave 0.424 grammes; it was more soluble in alkalies than fibrin, but composed like it of white globules, having the same general properties. The serum, gradually evaporated, weighed, when dry, 2.332 grammes; hot water dissolved 0.106 grammes of jelly from it.

As to the manner in which these phenomena were produced, it would appear that the soda contained in the fluids of the two first stomachs extracts albumen from the vegetables, and changes a portion of it into jelly: a view confirmed by the following experiment. To pure white of egg was added a solution of 2.424 grammes of soda, in 183 of water, the whole well mixed, and left exposed to air; as usual, it became a transparent yellow jelly. In 24 hours the whole had become fluid; carefully evaporated, it became brown; as it became more concentrated, some transparent insoluble films formed, and when the production of these had ceased, the whole was filtered through a cloth: further evaporation produced pellicles, which dissolved when stirred in the solution, and ultimately, when cold, the whole became a gelatinous mass, like that obtained from the second stomach.

The albumen in solution meets in the fourth stomach with free acid, which Dr. Prout has shewn to be the muriatic acid. This

acid is the second essential condition to digestion in vertebrated animals; without it no globules of chyle would be formed. In order to ascertain the part of the stomach by which the acid was secreted, that organ taken from a rabbit was emptied, washed several times with solution of soda, and then a cloth introduced, coloured by a vegetable blue colour; after six hours it was reddened principally near the middle region of the stomach, and several repetitions of the experiment gave the same result. Similar means indicated the same result with regard to the stomach of the sheep, &c.

The conclusions arrived at by the authors of the memoir are, that 1. The changes occasioned by digestion are purely chemical, and are not influenced by the vitality of the organs in which they take place: they may all of them, with the exception of those produced in the absorbent vessels, be imitated artificially by means of the fluids furnished by the secretory organs, namely, the soda and acid.

2. The soda is the agent to which the gastric juice owes those dissolvent properties which so much astonished Spallanzani.

3. The albuminous globules which, by their re-union form the chyme, are precipitated by the muriatic acid: this is a secretion of the fourth stomach in ruminating animals, and of the middle region of the stomach in vertebrated animals, in which this viscus is not subdivided.—*Bib. Univ.* xxvii. 229.

7. *White and Household Bread.*—Dr. Magendie tried the experiment of feeding dogs upon white bread and water, but all the animals died within 50 days, whilst those to whom he had given household bread, (pain de munition,) which only differed from the white bread by retaining a quantity of the bran, continued to thrive very well upon it. It is remarkable that one of the dogs that died, had been put upon his usual nourishment between the 40th and 45th days, but nothing could save him from the fatal effects of white bread.—*New Mon. Mag.* xv. 115.

8. *Properties of Margosa Oil.*—Mr. Allsop, in a letter from Madras, describes the oil obtained by expression from the nut or seed of the margosa-tree as having valuable medicinal properties, and acting as a preservative of perishable substances of various kinds. About 1½ ounce of the oil is obtained from a pound of the nuts. It (and also the leaf of the tree) is applied externally for pains in the joints, swellings, stings or bites of insects, *&c.*, and is a chief ingredient in the decoctions of the natives, for flatulency, indigestion, *&c.* Mr. Allsop was himself relieved from a very severe attack of lumbago by three applications of the oil.

The natives besmear their *kolays* or *cadjares*, on which their [illegible], histories, *&c.*, are written, with it. Some, upwards of two [illegible]ies and a half old, were nearly as fresh and in as good con-

dition as those recently taken from the tree. Mr. Allsop thinks that the oil applied to the shelves, sides, *&c.*, of bookcases, trunks, &c., will prevent insects or vermin approaching them, and would also be found useful in preserving cables, cordage, canvass, leather, or articles of any description, which are liable to be attacked by worms or other vermin.—*Tech. Rep.* vii. 17.

9. *Quantity of Rain which falls at different heights*—For phenomena of this kind observed by M. Flaugergues, see vol. xviii. p. 186, of this Journal. There are two rain gauges at the observatory at Paris, one on the summit of the building, the other in the court yard; the difference being 27 metres, or 86 feet. The quantity of liquid collected in them is never equal, the lower vessel always collecting more than the upper. The mean of eight years gives for the rain in the lower gauge 56.136 centimetres, and for the upper gauge 49.551 centimetres, so that the difference of 86 feet has occasioned an augmentation of one-eighth. The cause of this phenomenon does not seem referable either to the direction of the wind, or to the drops gathering water from the lower stratum of air, and is as yet inexplicable. The circumstance may be important at times in estimations made of the quantity of rain fallen at the same place in different periods.—*Ann. de Chimie.* xxvii. 397.

10. *Temperature on the Earth's Surface.*—From a general and extensive review of the various experimental data respecting the temperatures observed at different places on the earth's surface, the Editor of the *Annales de Chimie* deduces the following consequences:

In no place on the earth's surface, nor at any season, will a thermometer raised two or three metres above the soil, and sheltered from all reverberation, *attain the 37° of Reaumer, or 46° centigrade,* (114°.8 *Faht.*)

On the open sea, the temperature of the air, whatever be the place or season, *will never attain 25° Reaumer, or 31° centigrade,* (87°.8 *Faht.*)

The greatest degree of cold ever observed on our globe with a thermometer suspended in the air is 40° *Reaumer, or 50° centigrade below zero* (— 58°. *Faht.*)

The temperature of the water of the ocean, in any latitude, or at any season, never rises above 24° Reaumer, or 30° centigrade, (86° *Faht.*)—*Ann. de Chimie*, xxvii. 432.

11. *Heights of Mont Blanc and Mont Rosa.*—Mr. de Welden, after a very elaborate examination of the various measurements of Mont Blanc and Mont Rosa, gives the following as the results, which appear to be most accurate:

Mont Blanc . .	2461 toises, or 15737 feet
Mont Rosa . .	2370 toises, 2 feet, or 15157 feet.

12. *Medicinal Application of Leeches.*—At a sitting of the Academy of Sciences, M. Dumeril reported on a memoir of MM. Pelletier and Huzard, on leeches. The authors proposed to determine, in the first place, the causes which in certain cases rendered the wounds made by leeches very difficult to heal; and in the second to ascertain the circumstances in which particular leeches will not attach themselves to the skin to which they are applied. On the first point they are of the opinion of those who attribute the difficulty to the temperament of the patient, or to the nature of the disease, or to the imprudent custom which some persons have of disturbing and tormenting the animal in all sorts of way, in order to make it loose its hold, when it has been supposed to suck too long. On the second point they have ascertained, that frequently in commerce leeches are sent to the market in every respect resembling in appearance those which are known as medicinal leeches, but which, nevertheless, are entirely different in their internal organization. The false leeches have not the mouth furnished with cutting jaws, nor can they penetrate the skin of animals; their intestinal canal and stomach are differently formed.—*Ann. de Chimie*, xxviii. 96.

ART. XV.—METEOROLOGICAL DIARY for the Months of December, 1824, and January and February, 1825, kept at EARL SPENCER's Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For December, 1824.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Wednesday	1	39	39	29.07	29.20	W	W
Thursday	2	31.5	36	29.40	29.22	W	SE
Friday	3	31	35	29.22	29.40	W	W
Saturday	4	29	34	29.44	29.30	E	NE
Sunday	5	33	35	29.43	29.78	NNE	WbN
Monday	6	19	37	29.70	29.10	WbN	SW
Tuesday	7	33	38	29.30	29.37	W	W
Wednesday	8	34	43	29.65	29.57	W	SW
Thursday	9	36	42	29.57	29.48	W	W
Friday	10	28	43	29.70	29.01	W	W
Saturday	11	[illegible]	46	29.91	29.93	W	W
Sunday	12	40.5	49	30.08	30.17	W	WbS
Monday	13	44	49	30.18	30.25	WbS	WbS
Tuesday	14	41	47	30.29	30.20	WbS	WbS
Wednesday	15	42	49	29.93	29.72	WbS	WbS
Thursday	16	36.5	43	29.79	29.84	WbN	W
Friday	17	31	43	29.86	29.90	WbS	W
Saturday	18	40	49	29.98	29.96	W	WbS
Sunday	19	45	53	29.92	29.83	W	SW
Monday	20	46.5	50	29.21	[illegible]	SW	WbN
Tuesday	21	34	50	[illegible]	[illegible]	SW	SW
Wednesday	22	47	50	28.88	29.20	WSW	SW
Thursday	23	30	35	29.60	29.80	N	W
Friday	24	30	46	29.50	29.60	SW	NW
Saturday	25	40	53	29.33	29.27	W	WbS
Sunday	26	39	43	29.63	29.93	W	WbS
Monday	27	35	53	29.70	29.59	WbS	WSW
Tuesday	28	45	47	29.00	29.70	WbN	W
Wednesday	29	31.5	37	29.93	30.08	W	SW
Thursday	30	31	50	29.91	30.10	SW	SW
Friday	31	40	51	29.93	29.90	WbS	WSW

For January, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Saturday	1	47	50	29.87	29.87	WbS	WSW
Sunday	2	42	45	29.70	29.90	W	W
Monday	3	35.5	45	30.14	29.02	W	W
Tuesday	4	45	52	29.75	30.10	W	NNE
Wednesday	5	31	38	30.35	30.43	NW	N
Thursday	6	29	38	30.45	30.47	N	WSW
Friday	7	37	48	30.35	30.32	W	WbN
Saturday	8	32	[illegible]	30.55	30.62	NW	WbN
Sunday	9	32.5	40	30.70	30.70	NW	NW
Monday	10	36	37	30.70	30.71	NW	NW
Tuesday	11	34	41	30.61	30.58	WbN	WNW
Wednesday	12	34	40	30.53	30.50	W	NNW
Thursday	13	34	41	30.40	30.30	W	SW
Friday	14	39	45	30.22	30.18	WbS	WSW
Saturday	15	41	43	30.10	[illegible]	SW	SW
Sunday	16	37	44	29.60	29.45	SSW	S
Monday	17	32.5	40	29.68	29.80	WbS	S
Tuesday	18	35	47	29.20	29.21	SSE	WSW
Wednesday	19	31.5	40	29.28	29.30	W	W
Thursday	20	35	42	29.48	29.53	W	WbN
Friday	21	36	42	29.70	29.74	N	N
Saturday	22	34	41	29.80	30.00	NE	NE
Sunday	23	31	38	30.10	30.10	NE	NNE
Monday	24	31	39	30.00	29.80	W	SSW
Tuesday	25	35	39	29.52	29.78	W	W
Wednesday	26	28	37	29.67	29.81	S	SW
Thursday	27	38	50	29.68	29.68	SW	SW
Friday	28	34	43	30.41	30.57	W	WNW
Saturday	29	27	40	30.59	30.53	WNW	WSW
Sunday	30	34	48	30.30	30.28	WSW	WSW
Monday	31	37	50	30.36	30.30	W	WbS

For February, 1825.

		Thermometer.		Barometer.		Wind.	
		Low	High	Morn.	Eve.	Morn.	Eve.
Tuesday	1	46	49	29.95	[illegible]	W	WNW
Wednesday	2	33	43	30.27	29.86	W	SW
Thursday	3	44	49	29.30	29.40	WbS	W
Friday	4	28	33.5	29.50	29.60	WbN	W
Saturday	5	21	35	29.61	29.60	W	NW
Sunday	6	27	35	29.69	30.07	NW	NW
Monday	7	28	49	30.06	29.90	SW	SW
Tuesday	8	32	46	29.81	29.91	W	NW
Wednesday	9	32	45	30.19	30.27	W	W
Thursday	10	37	47.5	30.30	30.35	W	W
Friday	11	28	45	30.39	30.40	W	W
Saturday	12	31	45	30.40	30.40	WSW	W
Sunday	13	39	43	30.40	30.38	W	W
Monday	14	38	42	30.30	30.19	W	SE
Tuesday	15	39	40	30.03	29.91	SE	SE
Wednesday	16	38	45	29.87	29.85	SW	S
Thursday	17	32	47	29.85	29.78	S	WbS
Friday	18	42	44	29.80	29.97	SbW	NE
Saturday	19	41	47	30.08	30.08	E	SE
Sunday	20	39	49	30.10	30.13	SW	NW
Monday	21	29	46	30.30	30.30	NW	WbN
Tuesday	22	28	45	30.31	30.21	WbN	SE
Wednesday	23	37	40	30.10	30.08	SE	SE
Thursday	24	35	40	30.10	30.17	SE	E
Friday	25	29	39	30.27	30.26	E	NE
Saturday	26	32	37	30.14	30.00	NE	S
Sunday	27	33	43	29.63	29.46	S	NW
Monday	28	29	39	29.46	29.45	W	W

THE

QUARTERLY JOURNAL.

July, 1825.

Art. I.—*On a peculiar Vegetable product possessing the principal properties of Tallow.* By Benjamin Babington, M. B.

[Communicated by the Author in a letter to the Editor.]

Dear Sir,

During a visit to the coast of Malabar, in the year 1818, my attention was attracted by the very durable natural varnish which exudes from the Vateria Indica, a tree of common occurrence in those parts. Both the resin and the tree which produces it, have been noticed by the botanists of India *.

Dr. Ainslie's assertion, that this resin is soluble in oil of turpentine, is not quite correct, its habitudes being nearly similar to those of Copaul. It is not however to this material that I am now desirous of drawing your attention.

On a subsequent occasion, when employed at Mangalore in the province of Canara, I learned from a native of that town, that the same tree furnishes another product, which has not been noticed by any natural historian or European resident in the country, although its qualities are such, as may render it a valuable article

* *See* Dr. Roxburgh, MS. in Sir Joseph Banks's library; Ainslie's *Mat. Med. Hind.*, *&c.*

of commerce, and therefore seem to entitle it to the notice of our countrymen. The product to which I refer is a concrete inflammable, partaking of the nature of wax and oil, which, from its appearance, may not inaptly be termed a tallow. It is in use only in the town of Mangalore, and is there employed medicinally as an external application for bruises and rheumatic pains, and likewise, when melted with the resin of the same tree already alluded to, is used as a substitute for tar in paying the bottoms of boats. The method of preparing this material, is simply to boil the fruit in water, when the tallow is soon found to rise to the surface in a melting state, and on cooling, forms a solid cake. Thus obtained, the Piney Tallow (Piney is the native name of the tree which produces it) is generally white, sometimes yellow, greasy to the touch, with some degree of waxiness, almost tasteless, and has a rather agreeable odour, somewhat resembling common cerate. It melts at a temperature of ninety-seven and a half degrees, and consequently remains solid in the climate of India, in which respect it differs from palm or cocoa-nut oil; wrapped up in folds of blotting paper, and submitted to strong pressure, scarcely sufficient oil, or *elain* as it is termed by M. Braconnot, is expressed to imbue the inmost fold. Its tenacity and solidity are such, that when cast into a rounded form of nine pounds' weight, (in which state the specimen I possess was sent from India,) the force of two strong men was not sufficient to cut it asunder with a fine iron wire, and even with a saw there was considerable difficulty in effecting a division. Thus exposed to view, or still more obviously when a fresh fracture is made, it exhibits a crystalline structure, in small aggregated spheres, composed of radii emanating from a centre, not unlike the form of Wavellite. I learn that animal tallow, when melted into large casks and thus slowly cooled, assumes a somewhat similar appearance.

The concrete state of this inflammable, has probably been the cause why it has not become more generally known, even to the natives of those parts where the tree is indigenous, for this circumstance would oppose its being applied to the purpose of giving

light, since the inhabitants of India, as is well known, do not use candles, but lamps exclusively; for the supply of which they are furnished by nature with several fluid vegetable oils in abundance

I proceed to detail some experiments which I have made on this substance, with a view to determine its utility, as well as its composition and habitudes.

The specific gravity of Piney Tallow at the melting point, namely at 97½° Fahrenheit, is .8965, and at 60° is .9260.

Chlorine, when passed through it in the melting state, somewhat darkens its colour, changing it to a pale and dirty green. It also imparts to it a very remarkable odour, much resembling that of the rind of cucumber.

Alcohol, specific gravity .820, temperature, 55°, separated from 200 grains finely powdered, only 4 grains of a fat oil, which remained fluid at a temperature of 40°, thus confirming the result deducible from the exposure to pressure already mentioned, namely, that very little elain is contained in the substance. This alcohol, in repeated portions of which the powder was digested, until no more oil was taken up, also dissolved the colouring matter and the aroma; and on evaporation, these became united with the oil, leaving it of a deep amber hue and fragrant odour.

Alcohol, when boiled upon the melted Piney Tallow, besides this oil took up a small quantity of the less fusible part, which was deposited on cooling in minute tufts of a crystalline structure. The tallow thus left after the separation of the elain, had a greater tendency to crystallize, and to contract in cooling. It was nearly colourless and free from smell, and its melting point was raised to 99°.

The fixed alkalies digested on Piney Tallow form with it a saponaceous compound, but a practical soap manufacturer pronounces that he does not succeed in effecting a union, so as to form a marketable soap. It is however difficult, when operating on a very small quantity, to form an accurate judgment on this point, although, that it differs somewhat from animal tallow, probably in partaking of the nature of wax, seems pretty certain.

When manufactured into candles, it comes with facility from the mould, thus differing from wax, which does not readily admit of being cast; it gives as bright a light as tallow, and has the advantage of that material in being free from unpleasant smell, and in not emitting a disagreeable odour when extinguished. It unites in all proportions with wax, spermaceti, and tallow, and forms compounds with the two former, intermediate in their melting points, according to the proportion of their ingredients, and better adapted to the purpose of making candles, than the pure and more fusible substance itself.

With a view to ascertain the comparative combustibility of the Piney Tallow, candles of the materials under-mentioned were cast; one mould was used for all, and the wicks were composed of an equal number of threads. Having been accurately weighed, they were burned for one hour in an apartment in which the air was unagitated, and at a temperature of 55°.

	Weight in grains, when lighted.	At the end of an hour.	Loss
Wax - - - - - -	810	719	121
Half wax, half Piney Tallow - -	770	631	139
Spermaceti - - - -	760	601	156
Half sperm., half Piney Tallow -	777	625	152
Animal tallow - - - -	811	703	108
Half tallow, half Piney Tallow - -	792	681	111
Cape wax - - - - -	763	640	123
Piney Tallow - - - -	812	702	110

It should be mentioned that the wax candle in this experiment was an exception to the perfect similarity of circumstances required, its wick being much smaller; notwithstanding which, a greater quantity of the wax was consumed than in the case of the tallow, a result at variance with general opinion.

In the above experiment the wicks, with the exception just mentioned, were of the number of threads usually employed in tallow candles of the size used. In order, however, to judge of the Piney Tallow more accurately, as compared with wax and spermaceti, I had candles made in the same mould as before, with wicks composed of twelve threads, the number used in wax can-

dles of the size employed. It was also contrived to cast a wax candle for the sake of more perfect comparison.

The following are the results of an hour's combustion:

	Weight in grains, when lighted.	At the end of one hour.	Loss.
Wax - - - - - - -	730	594	136
Half Piney Tallow, half wax -	750	622	128
Spermaceti - - - - - -	736	590	146
Half wax, half sperm. - - -	762	616	146
Piney Tallow - - - - - -	774	684	90

From these two sets of comparative experiments, which, allowing for the smaller size of wick in the latter, coincide tolerably well, we are led to conclude, that the Piney Tallow approaches nearer to animal tallow in its rate of combustion, than to any of the other substances employed; as indeed we might be led to suppose, from its melting point being nearly the same.

It may be objected to these experiments, that they do not admit of sufficient precision to enable us to come to any accurate conclusion, and it must be admitted that they are only approximations to the truth. That I might be enabled to judge, however, to what degree of probable error I was liable, I had six candles of the best animal tallow, cast in the same mould as the rest, with wicks of twelve threads, and these I burned for one hour as before, using the same precautions. I first performed the experiment, snuffing them every ten minutes, and then without snuffing them at all, being desirous collaterally to ascertain what difference in the combustion the snuffing would cause.

The following are the results:—

Snuffing every ten minutes.

ns, when lighted.	After one hour.	Loss.
781	675	106
782	682	100
784	682	102
785	681	104
785	676	109
792.5	690	102.5

Without Snuffing.

Weight in grains, when lighted.	After one hour.	Loss.
673	573	100
676	573	103
676	570	106
681	581	100
681	580	101
689	592	97

It thus appears, that the maximum of difference has been nine grains in both sets of experiments, while the average in the former is 103·91 grains of loss, and in the latter 101·16. The degree of approximation to accuracy in the former experiments may be thus estimated.

I may incidently add, that it also appears that the consumption of material in a tallow candle, snuffed at intervals of ten minutes, is only 2·75 per cent. more than in a candle not snuffed, a difference very inconsiderable, compared with the difference of light produced.

The ultimate analysis of Piney Tallow being a point of some interest, much pains were taken to perform it with accuracy, and with the aid of my friend, Dr. Dowler, I succeeded in two separate experiments, in obtaining perfectly similar results, which I have so far reason to believe correct. The analysis was performed in a glass tube, by means of peroxide of copper, in the usual method, and the water formed was detained in an acute bend of the tube, filled with a given weight of calcined rock crystal, coarsely powdered; and immersed in a mixture of ice and salt. At the close of the process, this portion of tube was cut off, carefully corked at both ends, and immediately weighed. The water was then driven off by the heat of a spirit lamp, and the tube again weighed. I have thought it worth while to mention this method of collecting the water, as I am persuaded that it is more accurate thus to use mechanical means in detaining it, than to employ muriate of lime, or other chemical absorbents, which attract moisture so easily, as to make it diffi-

cult to weigh them, or to bring them twice to the same degree of dryness.

Result of the ultimate analysis of three grains of Piney Tallow:

Carbonic acid, 18 cubic inches, water, 3·30 grains:

Hence the three grains consist of

Carbon	2.31	=	.770	=	10 atoms.
Hydrogen	.37	=	123	=	9 atoms.
Oxygen	.32	=	107	=	1 atom
	3.00		100		

In a letter from Father D'Incarville, at Pekin, published in the *Philosophical Transactions* for 1753, a somewhat similar substance is described as the produce of China. His words are "the berries of the Tallow-tree are of great use in the southern provinces, where there are very few sheep. Almost all the candles sold there are made of the oil drawn from these berries. They procure this oil in the same manner that I have mentioned concerning the wax, (which is procured by boiling the matter rasped off the branches of the tree) and as this oil is not of so good a consistency as tallow, for its cohesion when candles are made of it, they dip them in the white wax mentioned. "The external coat thus made, prevents them from guttering. At Pekin, the same thing is done with tallow candles."

That the Chinese vegetable tallow, and the Piney Tallow, are not the same substance, appears from the remark that the former is not of so good a consistence as mutton tallow, whereas the latter is decidedly harder, which is one of its advantages over that material.

By recent inquiry, I have ascertained that 500 cwt. of Piney Tallow may be procured in the town of Mangalore for fifty rupees, which is somewhat more than 2d½ per pound. At this moment it appears that not more than two tons could be purchased, since if we except the trifling quantity consumed as a medicinal application, it is only applied to one purpose, and manufactured only in one town. The tree, however, is so common throughout the western coast of the Peninsula of India, at least as far northward as the Moun-

daries of the Province of Canara, that though the fruit is not now turned to account, it would no doubt be brought into use were a demand created for its valuable product.

It would be out of place to enter into any detailed speculations respecting the eligibility of introducing this substance as a material for candles. It may however in general terms be stated, that it could be imported into this country at less than one-fourth the price of wax, and though it does not possess all the advantages of that substance, it is still considerably superior to animal tallow.

ART. II.—*Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by* William Thomas Brande, F.R.S. Sec. R.S., and Professor of Chemistry in the Royal Institution, *&c.*

[Continued from page 92.]

III.

GEOLOGY, like most other branches of study, must be considered in its practical, and in its theoretical relations, the first teaching us the relative positions, aspects, and qualities of those mineral aggregates, or rocks, which incrust the solid nucleus of our globe; and the second aiming at the discovery of the causes by which they have been produced, and investigating the events which have attended their deposition, and influenced their arrangements and characters.

In either view of the subject, geology presents an interesting field of inquiry, and when observation is blended with theory, when we are permitted to relieve the drudgery of collecting facts by incursions into its speculative regions, it opens a train of study and of research, to which few, from its variety and importance, can be inattentive or indifferent.

Whatever temptations may be held out, to deviate into the contrary path, and to amuse ourselves with the hypothetical flights of the numerous writers who have indulged in the speculative department of our subject, I propose, as far as may be, to confine myself in these lectures, to details almost exclusively of a

practical nature, and if I sometimes venture to put a theory before you, it will rather be to show its failings and defects, than to offer any thing towards its embellishment and support; for it will be found, that in geology philosophers have too generally commenced their inquiries in the *study*, instead of in the *field;* that the most visionary and preposterous notions have been sanctioned by great names, and supported by most able controversialists; and that personal and acrimonious disputes have been suffered to ruffle the calm of philosophical society, which might have been speedily and amicably settled, by a reference, not to books of theorists recording opinions, but to the book of nature, establishing the fact. It is, I think, chiefly to the labours of modern and even contemporary geologists, that we owe the new and improved features which this branch of natural knowledge is acquiring. In their publications they have candidly recorded facts, which have shown the futility of much that was before thought sound argument and reckoned as brilliant reasoning; and there has therefore of late, in our hemisphere at least, been a proportionate and promising defalcation of those writers, who though, on some occasions, profound and ingenious, are not consistent with nature; c' 'ho have misemployed their abilities in assimilating facts to theory, instead of adjusting theory to facts.

Geological theory, however, has, if properly estimated, many and important advantages, and many of the geological facts which have been established, as well as of our most valuable practical documents in illustration of the structure of the globe, or the stratification of particular districts, have (as I hinted in my introductory lecture) been derived from those whose curiosity has been incited to the inquiry, by the stimulus of theoretical discussion; this indeed is universally the case in the progress of experimental science; and although there are still among the philosophers, a few who are credulous in respect to all that *confirms* their preconceived opinions, but sceptical upon the facts that *oppose* them, the greater number have shown their willingness to exchange opinions for truth, and to eradicate error wherever it occurs, and however exalted the name or character by which it may have been planted.

As, then, I trust to find a sufficiency of facts, and those interesting and important, to form the basis of these lectures, I shall at once proceed to lay before you a brief account of the structure of the earth's surface, without stopping to inquire whether the globe we inhabit be an extinguished sun, or a mere pimple brushed from the face of that luminary by the tail of a comet: whether as Kepler thought, we are parasitic animals, sporting upon the exterior of a huge leviathan whose nostrils are volcanoes, and whose perspiration constitutes the ocean: or whether we may assume the mineralogical conjecture, that the earth is a monstrous crystal, the sides and cleavages of which represent the strata of its surface.

The strata of the globe (for notwithstanding the anathema with which Mr. Greenough opens his *Essays on Geology*, I shall be bold enough to use that term, under the presumption of its general intelligibility) appear from the concurrent testimony of travellers, to present everywhere upon its surface, certain analogies in respect to arrangement, and to succeed each other in a certain definite order. In the valleys, and we will assume the valley of the Thames, and the greater part of Essex, as an instance, we find gravel, pebbles, sand, and other matters resulting from detritus, mixed with more or less of vegetable remains, and constituting soils of various degrees of fertility and extent. The soil being removed, we find clay and sand resting, probably everywhere, but evidently in many place upon chalk, and containing such a remarkable assemblage of organic remains, some of vegetable, and others of animal origin, as almost to baffle all conjectures as to whence they came, or under what circumstances they were brought together. The remains of sea animals are blended with those of the land, quadrupeds with fish, and fresh-water fish with those peculiar to the ocean. Animals of the land, the air, and the water, are assembled together in most unaccountable incongruity; fruit and leaves, hazle nuts and pine cones, are mixed with shark's teeth, crab's claws, and oyster-shells. No less than five hundred varieties of fossil-fruit, mixed with sea-shells of various descriptions, have been found in the clay of Sheppey Island: and Mr. Trimmer's brick-fields at Brentford have yielded

such a remarkable collection of sea-shells, shark's teeth, bones of the elephant, hippopotamus, ox and deer, together with fresh-water shells, as to impress us with the idea of the destruction or relics of a vast *menagerie*, in which animals of all denominations, and from all quarters of the globe, had been associated.

The clay formation of London and its vicinity, is thrown into many picturesque irregularities, which are hidden by the buildings of the metropolis, but which are seen in Shooters Hill on the east, in the hills of Hampstead and Highgate on the west, and which contribute to the unrivalled beauty, in this line of prospect, of Richmond Hill in the west.

In the Isle of Wight, at its west extremity, there is a section of the deposits superior to the chalk, attended however by some peculiarities to which I shall advert more in detail. They are especially remarkable for furnishing alternations of marine and fresh-water shells, from which it has been *conjectured* that the spot has been subjected to alternating inundations of the sea, and that in some part of the intervening period, it has formed the bottom of a freshwater lake.

If we travel from London to Paris, we find, in the vicinity of that metropolis, facts equally irreconcilable with all commonly adopted theory, and in most respects resembling those which the London *basin*, as it has been called, exhibits; for, if we suppose a large cup or concavity, scooped out of the chalk, imperfect in certain parts upon the sea-coast, where sections of its contents are exhibited, and filled up with sand, clay, organic relics, and alluvial products, we shall form a tolerably accurate notion of the general character of these districts.

In the Paris *basin* then, the first substance that occurs, lying immediately upon the chalk, is a layer of plastic clay fit for the manufacture of pottery, and upon this is a coarse limestone, with beds of sandstone and marle, enclosing marine petrifactions of various kinds, and many shells which still retain their pearly lustre. This stratum, however, is not quite continuous, for, in some places, its space is occupied by a siliceous limestone without any shells. We may, however, from the position and contents of this stratum,

call it the *lowest marine formation.* It is covered by a bed of gypsum, and by one of marle, enclosing a very remarkable series of organic remains, among which we may particularly notice, petrified wood of the palm kind, and the relics of shells and of fishes apparently of fresh water species. But, in the gypsum, are abundant remains of amphibious animals, and birds, including the bones of an extinct species, varying in size from a sheep to a horse; there are also the bones of an unknown species of dog, fox, and ichneumon; the bones of the pelican, starling, and quail tribes; of the crocodile and tortoise; and of several varieties of fish allied to the present species that inhabit fresh water.

Above these beds, the contents of which are thus miscellaneous and remarkable, and which are supposed to bear traces of *fresh water* deposition, we find two beds of oyster-shells, one of which exactly resembles in the appearance of the shells, and in their disposition, those which are usually found in the ocean; the greater number of them are whole, and have both valves; they alternate with sand and sandstone, and are covered by a deposit of limestone, containing fresh water and land shells, nearly all of which belong to the genera now living in morasses; lastly, we come to the uppermost deposit, containing rounded stones and pebbles in a mixture of sand and clay, and abounding in the fossil remains of large trees, with the bones of elephants, oxen, deer, and other large animals *.

That low and level countries, in general, exhibit the same extraordinary records of devastation and change appears probable from various geological investigations that have been made in several parts of Europe, more especially in France and Germany; and in some districts of North America, which have been geologically explored, similar evidence of analagous changes is not wanting.

We may now revert to the strata lying upon the chalk in the Isle of Wight, in order to examine a little more particularly the circumstances under which they appear to have been deposited;

* *Vide* CUVIER and BRONGNIART.

and here the *verticality* of certain strata in the neighbourhood, which are usually horizontal, and which here exhibit traces of *having been horizontal*, are not among the least remarkable of the phenomena which this district exhibits. Headon Hill, situate at the west end of the Isle of Wight, in Alum Bay, is about four hundred feet high, and exhibits, towards the ocean, a curious and distinct section of its strata. Its base, as there seen, is a fine white sand, which may be traced into Totland and Colvill Bays on the north, and which, from its purity, is in great request among the manufacturers of flint glass.

Upon this is a bed of gray clay, in which selenite and fossil shells occur, and which supports the lower fresh-water formation of this district; that is, it has lying upon it a series of marles, some of which appear to consist of fragments of shells, with a few entire portions, from which their species have been ascertained. To this succeeds another stratum, containing sea-shells in great abundance and perfection, and separated by a bed of sand a few inches thick, from the upper fresh-water formation, composed of a marle, abounding in fresh-water shells, and covered by the alluvium, which forms the summit of the hill *.

Having now adverted to a few of the most remarkable facts relating to the uppermost strata of alluvial matters, it may, perhaps be expected that I should consider the various discussions to which they have given rise, and the opinions that have been maintained respecting the origin of such strange events as are thus presented to us at our very entrance upon the examination of the earth's strata.

It has been frequently argued, that the deluge is quite sufficient to explain all the remarkable assemblages of the remains of birds, beasts, fish, and vegetables, we have just had occasion to notice; and to account for the occurrence of the elephant, the crocodile, and other animals of that description in these northern parts of our hemisphere, it has been conceived, that they were washed from tropical climates, and brought to us by a great and

* Vide WEBSTER, in *Geol. Trans.*, and Sir H. ENGLEFIELD's *Description of the Isle of Wight.*

overwhelming current of water, traversing the globe in this direction; but these very theorists tell us of the formation of gravel; of the detrition and rounding down of the hardest substances in nature, by the same torrent, and yet the organic remains we have been looking at are for the most part so perfect, that they retain all those protuberances and indentations by which the skilful eye of the anatomist not only detects the part of the body to which the bones belong, but discerns in the majority of cases, the genus and species of the animal, by the inspection of a single bone. Hence it has been inferred, but the inference, as we shall see, is not without most weighty objections, that these animals lived and dwelled upon the very spots in which we now find them. The bones of elephants and other very large animals, some approaching to, but others widely different from the species now in existence, have been met with in almost all countries where they have been diligently looked for, either in caverns peculiar to certain rocks, or in the alluvium of valleys, and generally, therefore, either in, or not far from the beds of rivers, and in islands as well as continents. Marsigli, in his *History of the Danube*, has described the remains of elephants, supposed to be those which Trajan carried with him in his expedition against the Dacians. At the beginning of the last century, nearly a hundred tusks of elephants were dug up in an alluvial soil in Wirtemburg, some of which were upwards of ten feet long, and they were accompanied by the teeth and bones of several unknown animals, or extinct species.

In Italy, the country about Verona is famed as the repository of organic relics; bones of enormous magnitude have there been exhumated, and in such a perfect state, that we can scarcely suppose them to have travelled far, much less to have been submitted to the continuous operation of, or to ordinary transportation by water. No wonder that the ancients thought these were the remains of a giant race; that Pliny talks of human skeletons, twenty-four feet high; that Kircher describes the skeleton of Pallas, slain by Turnus, as higher than the walls of Rome; and that of one of the Cyclops, probably Polyphemus himself, as having been somewhere about 300 or 350 feet high. The same author,

and others of great respectability, give us the measures of other colossal personages, and when we reflect, that the credulity and misinterpretation that are here so glaring, are not the errors of the weak and illiterate, but of men of learning, and men of talents, of the best instructed by reading, by conversation, and by travel, of any in the ages in which they lived, we cannot but be struck by the difference between the criterion of truth, as received in those ages, and at the present day; a diversity referrible to divers causes, but to none more than to the progress of natural and experimental science, in modern, and more particularly in our own times.

That the animals, of whose remains we are now speaking, actually occupied the spots upon which they are found, and that their skeletons were not transported thither by the deluge, or by any post-diluvian debacle, is by some supposed to be rendered additionally probable, by the discovery of a rhinoceros and of an elephant in the north of Siberia, with part of the flesh and skin preserved. In regard to the former, Pallas observes that its foot alone was coated with more hair than that usually found upon the whole body of any living rhinoceros; and hence he suggests the probability of the animal having been a native of a colder clime than the Torrid Zone, and it is known that the rhinoceros in the North of India has more hair than the animal dwelling in the South of Africa. Again, in the year 1799, there was discovered in an iceberg, on the North shore of Siberia, a singular mass, which the fishermen observed for several winters without being able to ascertain what it was, when in 1803, in consequence of the thawing of the ice, it became evident that it was a large elephant-like animal, of which one of the tusks was protuberant. We are indebted to Mr. Stokes for an account and drawing of the skeleton of this animal*; the tusks were of extraordinary size and beauty, and as the proprietor was content with the profit they afforded, he was heedless as to the carcass, the greater part of which served as a repast for the bears, wolves, and foxes, who seem prodigiously to have relished

* Vide Vol. VIII. of this Journal, page 95.

this anti-diluvian delicacy. The skeleton is about 9½ feet high, and 16½ long, and is now in the Museum of Petersburg. It was covered with hair in such abundance, that Mr. Adams, who rescued the remains from destruction, found no less than 36 pounds weight of it left by the beasts of prey that had devoured the flesh.

While upon this subject, it may not be irrelevant to observe, that almost all the ivory turner's work made in Russia, is from the Siberian fossil ivory tusks, many thousands of which are annually obtained on the banks of the larger rivers of that mighty empire.

I have adverted to the existence of fossil bones in various parts of America; those from the Ohio are particularly worthy notice, and were a long time confounded with the mammoth or fossil elephant of Siberia. But as Dr. Hunter long ago observed, and as M. Cuvier has more lately remarked, though there are strong resemblances, there are also marked differences in the skeleton, and the teeth especially are more those of a carnivorous than graminivorous animal; the enamel is external, and the shape and structure of the teeth such as to have fitted the animal for tearing up flesh, rather than masticating vegetables; they are also peculiarly tuberculated: hence Cuvier, referring this animal to a distinct genus, has called it, *Mastodonton;* he has, I believe, ascertained the existence of five or six species, some of which are found in the Old World. I have adverted to the arguments in favour of the existence of these animals upon the spots in which they are now found, and have shewn reasons for supposing them there domiciliated; but to render such an opinion plausible, it must be proved that the climate of the northern parts of the world was once much warmer than at present, or that the animals themselves were endowed with very different temperaments, and we are far from being able to substantiate either of these requisites.

Against such an opinion it has also been argued, that very abundant remains of the same description have been discovered in islands, and even in some of the smaller isles of the Mediterranean, which would scarcely afford an elephant food for a week, and thus it has been supposed that the idea of these animals having

been at home on these islets is preposterous and absurd; but then again, it is manifestly not impossible, that when the islands in question were inhabited by these larger animals, they were parts of a continent, and it is not difficult, as I shall show by-and-by, to urge strong grounds in support of such a theory.

Another circumstance has been urged against the notion that these bones have been transported from other countries, which is their very general diffusion, and their abundance in various situations as well as climates; had they only occurred in countries conquered by the Macedonians, the Romans, and the Carthaginians, and were they the bones of the elephant only, and further, did they present no peculiarities that stamped them as a more ancient and an extinct race, we might suppose, with the earlier speculators, that they were the bones of animals which had perished in the warfare of those nations, but taking the facts before us into the account, it is perfectly absurd to regard them as the victims of the restlessness and ambition of the human race, and they seem (as a modern writer has suggested) to belong to a period when man's dominion over the earth was limited and feeble; when perhaps the human race was confined to some favoured spot, and when the elephant, from his sagacity and strength, "was the chief master of the earth."

Among the extinct species of carnivorous animals, we must not forget to mention the bears, whose remains are found in the caves of Bareuth and the Hartz; the bones discovered by Mr. Whidby, in certain caverns in the limestone of Plymouth probably belonged also to bears. Nor must we omit the singular associations of bones described by Mr. Buckland in a cave at Kirkdale, at Kirby Moorside in Yorkshire, among which those of the hyæna, bear, wolf and fox, of the elephant, rhinoceros, hippopotamus, horse, deer, rabbit, and water rat have already been discovered, presenting a truly curious assemblage, and importantly connected with various objects of geological inquiry. From the apparently gnawed condition of the bones, it has been concluded that the den was inhabited by hyænas, and that the other animal remains are the bones of their prey which had been dragged into the recesses of

the den. Cuvier seems inclined to suppose, that the era of these animals was of a later date than that of the mammoth, and as there is no appearance of any sudden catastrophe having attended their sepulture, and as they are unaccompanied by the bones of marine animals, he also thinks that they lived and died in the caverns which now contain their remains. "Each cavern, it has been supposed, in the extensive chain of the Hartz and Hungarian mountains, was the den of a single despot, who sallied forth to prey upon the defenceless inhabitants of those woods which in later times, after men had become masters of the world, were called the Hyrcinian Forest."

Perhaps the most remarkable unknown species of fossil animal, is the elk of Iceland, to which I ought before to have adverted; it differs from our present elk, and from the rein-deer, in the size and conformation of its horns, the largest horns of living elks not being more than half the size of those found in a fossil state. But Mr. Weaver has rendered it probable that these remains are not antediluvian.

But if there be a difficulty, and a great one there is, in accounting in any plausible manner for the accumulations of bones and remains of animals of species now extinct, and in climates and countries apparently foreign to their habits and constitutions, there also are great difficulties in framing a theory to account for the alternations of salt and fresh-water shells, and for those prodigious accumulations of pebbles and rounded fragments constituting the beds of gravel that are incumbent upon the clay, and of which London and its vicinity presents us with such numerous and often interesting instances.

I have already adverted to the notion of the alternate inroads and retreats of the sea, combined with the occasional existence of fresh-water lakes, as having been assumed as the most easy method of accounting for the alternations of shells and deposits that have been discovered in the vicinity of Paris, and in the Isle of Wight; but this hypothesis is open to unbounded objections; the beds containing the different deposits closely resemble each other; the marine limestone and the fresh-water limestone, the marine clay and the fresh-water clay, and the marine grit and the fresh-

water grit, present no mechanical or mineralogical difference. We may, therefore, perhaps, be sceptical, and there is much ground for scepticism, concerning the supposed accuracy of distinction between river and sea-shells: the common test is the thickness, and an extremely delicate and thin shell is, merely as such, regarded as a fresh-water deposit where it happens to fall in with hypothesis; but we well know, that sea-shells are by no means uniformly thick; nor are river-shells always thin and delicate. Mr. Greenough, therefore, is justified in doubting the possibility of the depositing menstruum having changed without any corresponding change in the deposited matters; and in discrediting the probability of a sea having retired before a lake, or a lake having been overwhelmed and annihilated by the inundation of the sea, without any trace of such catastrophe being any where visible, on the then and still unconsolidated and unresisting materials which furnished the scene of action. But the *alternation* is the principal difficulty, for the existence of lakes at levels and in situations which they now no longer occupy, is rendered probable by the characters and structure of the sides of hills and mountains, that bound certain valleys; and more especially by the very remarkable appearances, constituting what are termed the *parallel roads* of the mountains of Lochaber. The very extraordinary aspect of these ridges, is such as to arrest the attention of the most incurious spectator, and we cannot wonder that the solitary and poetical highlander should attribute to the ideal and gigantic beings of former days a work, which scorning the mimic efforts of the present race, marches over the mountain and the valley, and holds an undeviating course over crags and torrents.

These ridges or roads are seen in three strong lines on each side of a long, hollow, and deep valley, at a considerable elevation, and corresponding exactly with each other. Among the notions of their origin some are purely imaginary, others may be hypothetically supported. It has been supposed that they were made for the pleasure of certain kings of Scotland, who resided at Inverlochy Castle, and near these are the kettles of Fingal, in which with equal probability the royal personages are supposed

to have dressed their venison. But although the magnificence of the object is such as to heat the imagination, and impose upon the judgment when considered as a work of art, we must look to some natural operation for the origin and cause of the phenomena, and among these none more plausible than the idea of the lake having successively occupied the different heights in the valley of these roads, and having formed them by the continual action of its waters, the edges and boundaries of which they represent; of such phenomena we shall offer more decided evidence in a future lecture.

The depositions of gravel so common in the London basin, and more especially upon the north of the metropolis, are very generally admitted to be of aqueous origin, and it has been supposed chiefly to have arisen from the wearing away and demolition of strata once lying above these alluvial matters, and especially from chalk flints rounded by the attrition of those waters which broke down and washed away the chalk that contained them; there is, however, the most distinct evidence of the more remote original of these beds, for they contain pebbles very unlike those flints now found in chalk, and they often present pieces of granite, quartz, and other matters, not only not existing in the neighbourhood, but very unlikely ever to have formed strata lying *above* our present chalk beds. Some more extensive, violent, or general cause, therefore, must have operated, as indeed is rendered evident by the inspection and situation of those larger and scarcer masses of rock, evidently rounded and transported, which are called *boulders;* these were probably much more abundant in former times than at present, having been removed partly for the purpose of building, and partly in clearing land for cultivation, but how they arrived in their present situations, often far distant from their evident sources, and not unfrequently with hills and valleys intervening, is a question which can only be answered by supposing what may perhaps be regarded an extraordinary and unwarrantable stretch of hypothesis; namely, that these boulders were rolled into the places they now occupy, by some tremendous current and inundation, carrying sand, gravel, and boulders along

with it, and which occurred before many of the hills and valleys that now lie between the sources of the boulders and their present situations were formed and excavated. That our present torrents could never have moved these mighty masses, considered independent of their origin and localities, is quite obvious; and hence some have been driven to the necessity of assuming a sudden disruption of the chaotic ocean, attended by earthquakes, which rent the strata, and loosened the masses that we now find transferred to places far distant from their original homes; but the event of the deluge will amply suffice as the cause of these phœnomena.

Certain it is that no powers now active can be considered as efficient for the production of our beds of gravel, and much less for the transportation of boulder stones, and that we *must* go back to a different aspect and state of the earth's surface from that which it now presents, and that we must suppose planes to have existed where the surface is now irregular. To illustrate this position by actual occurrences, we mus suppose that the blocks of granite met with in the recesses of Mount Jura, and which if we consider them as derived from the source nearest at hand, must once have formed part of Mont Blanc, attest the non-existence of the Valley of the Rhone, and of the Lake of Geneva, at the time of their transportation. That the parasitic gravel and soil of the island of Malta attest the non-existence of the Mediterranean at the time they were there deposited. That the blocks of primitive Norwegian rocks that are scattered over the north of Germany, Russia, and Holland, and occasionally met with on the east coast of England, announce the non-existence of the Baltic and German Sea while these blocks were in motion. And lastly, to come nearer home, the beds of pebbles and pieces of granite and quartz that constitute the diluvium deposited upon the little island of Staffa, could not have resulted from the flow of water in the present state of things, but must be referred to an antecedent period when Staffa formed part of Mull, or when the whole was perhaps a promontory of the main land. If we imagine, says Dr. Mac Culloch, the origin of the alluvial matter to be in Mull only, it still proves great changes; if we suppose, as some *have* sup-

posed, that Staffa, as it now is, was lifted from the bottom of the ocean, with these pebbles and this alluvium lying upon it, we do not diminish our difficulty; we assume changes greater than the former, and adopt causes less consistent with the effect. Whether the forces that have operated have been gradual or rapid, slow or sudden, are questions which perhaps may be answered by an examination of the strata and their arrangements, and as we find *these* apparently undisturbed, it is not likely that the separation of Staffa from the adjacent islands should have been effected by any great dislocation, or earthquake, or Huttonian force emanating from below; it seems to have been more gradual and tranquil, and to have resulted from powers which have not shaken or disturbed the neighbouring parts, but yet have cut and carved away the intervening rocks. I must however beg that none of these remarks may be considered as having any reference to the origin of the island itself; but merely to the probable source of the matters which lie upon its basaltic strata.

If we now reflect upon the wonderful occurrences which even our most superficial strata record; upon the extinct species of quadrupeds, birds, and probably of vegetables, which they contain; and upon the enigma which the mere inspection of a gravel pebble calls upon us to solve; we shall not be surprised at the occurrence of greater difficulties, when we descend into those strata of the earth, of an origin more remote and recondite; and I trust that my hearers, seeing the insufficiency of theories, even when applied to the simplest cases, will not be disposed to consider that I have judged amiss, if I pass lightly over the speculative part of geology, and limit myself chiefly to its less excursive and entertaining, but more profitable practical details.

ART. III. *Description of* Psittacus Fieldii, *a new species of Parrot from Australia. By* William Swainson, F. R. and L. S.

IN my account, recently published, of several additions to the Ornithology of Australia, I have not noticed a most beautiful

Parrot, of a form and species totally dissimilar from all those hitherto received from the southern hemisphere. To the liberality of my friend Barron Field, Esq., I am indebted for the only specimen, so far as I can learn, now in this country; and it is in justice to the scientific acquirements of that gentleman, that I here commemorate it by his name.

The superb family of *Psittacidæ*, wherein nature has united all that is lovely in colour and graceful in form, has engaged the attention of two eminent ornithologists now no more, MM. Levaillant and Kuhl. The latter has, with great judgment, divided the numerous species it contains into natural and geographic groupes; and my friend Mr. Vigors intends shortly to investigate and arrange the whole family according to the quinary system of Mr. M'Leay: the subject is inviting, and cannot be in better hands: I shall therefore merely give such a description of the bird before me, as may enable others to ascertain the station it may be found to occupy among its congeners.

PSITTACUS Fieldii.

Rufous-headed Parrot.

P. viridis; capite castaneo-fusco; alis infrà nigris; tectricibus interioribus cæruleis; caudâ rotundatâ.

Green; head chestnut-brown; wings beneath black, under wing-covers cœrulean-blue; tail rounded.

In size, this bird is rather larger than the Ceram Lory; the bill is comparatively thick and strong: the upper mandible has a slightly sulcated line down the middle of the culmers; while the under mandible is longer than it is deep; the gonix ascending, and the tip thick and obtuse, like that form seen in the short-tailed parrots of the New World; the under part is obsoletely triangulated; the cere is entirely naked, and the aperture of the nostrils very large, and perfectly round. The entire plumage of the head and ears is a deep red or chestnut-brown; much paler on the lower part of the cheeks and chin, where it becomes tinged with green. All the remaining upper plumage is of a rich and change-

able grass-green, in some lights tinged with golden yellow, and in others with brown. The under plumage is paler and more inclined to yellow. At the base of the wings, adjoining the scapulars, there is a small obscure reddish spot. The quills on their outer surface are dark green, but on the inner surface dusky black; while the inner wing-covers, and the feathers on the sides of the body immediately adjoining them, are of a brilliant sky-blue. The second and third quill are very slightly longer than the first. The tail is of a moderate length and rounded, and the extremities of the feathers are ovately or obtusely pointed: the colour above is green, with the interior webs yellowish; and this last colour predominates on the under surface. The tarsi are black, and comparatively short.

Art. IV.—*On the Origin, Materials, Composition, and Analogies of Rocks*, by John Mac Culloch, M.D., F.R.S.E.

[Concluded from p. 41.]

Of the Analogies among different Rocks, and of their resemblances to Unconsolidated Strata.

Having thus attempted to assign probable causes for the consolidation of the original materials of rocks, it will not be useless to attempt to trace these through their progress; to inquire if it is possible to discover, in the component parts and disposition of the most ancient rocks, any resemblance to the loose matters which are now daily deposited beneath the waters of the present earth.

On the subject of the unstratified rocks, little can here be said in addition to the remarks on their origin which have often been made; but as these must have been formed out of previous rocks, and not from original materials, it will be better to postpone any observations on them till the last.

If we examine the deserted seat of an inland lake, we discover beds of compact mud intermixed with leaves, or of mud with land shells, or of sand, or of peat, or of all these, in one or more series of alternations. Abstracting the question as it relates to peat,

we have here an analogy to the rocks of a coal series. The mud with its plants, or shells, represents the different shales and limestones; and the sandstone is the counterpart of the sand bed. The whole requires consolidation only, to render it an ordinary series of rocks.

In sinking through the ancient estuaries of the sea, long filled up and converted into dry land, similar beds of mud and clay, of marine shells entangled in mud, and of sand and gravel, are found; varying in number, in thickness, in the order of repetition, and in the quality or nature of the remains, in almost every place. It is unnecessary to point out more distinctly a similar analogy in these preparations for a series of secondary rocks; but it obviously requires only a repetition of the same deposits, sufficiently frequent, to produce the whole series of secondary strata. At what period the act of consolidation may have taken place, we have no means of knowing; yet, as far as our observations have hitherto reached, we have no reason to think that any extensive operations of this nature are now going on, excepting those formerly mentioned. The process may possibly be too slow to fall within the sphere of our investigation.

If the more ancient strata have been formed from similar materials, they should possess an analogy to the secondary; and, admitting such differences as may be accounted for by the circumstances of difference in respect to consolidation to which they have been exposed, we should, among them, find a series of alternations analogous to the sandstone, shale, and limestone of the latest series. Such an analogy can indeed be traced, but it is imperfect. It will, in fact, be seen, that a great part of these differences is explained by admitting the effects of heat on them; and it may fairly be presumed that there have also been differences in the rocks from whence the materials of these strata were originally deposited. But there is still one difference remaining, of great importance, and on which some light may at least be thrown, if it cannot be fully explained. It consists in the very great disproportion of limestone in the two series; that rock being

abundant in the later, and comparatively rare in the earlier strata.

The formation of coral islands, proves that enormous and solid masses of calcareous rock are the produce of animals alone; and when we reflect on the magnitude of some of these, we have no reason to be surprised at the extent of those rocks which, among the secondary strata, are composed chiefly of shells. Were we even to suppose that every particle of the largest bed of limestone known, was originally part of the body of a shell, we should, as far as the bulk of the mass is concerned, assume nothing that would not be countenanced by the magnitude of the great coral reef of New Holland. If the most minute animals of creation can thus, by their numbers, execute unassisted works of such enormous magnitude, and, as navigators think, within spaces of time comparatively limited, it is far from unreasonable to believe that the succession through unnumbered ages, of animals so far exceeding them in bulk and in the relative quantity of their calcareous produce, should have generated all the calcareous strata in the secondary series.

It is not necessary here to ask whence the calcareous matter has been derived, or to suppose that it is an animal product. The difficulty is, at present, unquestionably insurmountable; but, in this case, it is of no moment. It can form no objection to the power of oysters or pectines in producing, by their own energies, a bed of limestone; because the fact, however inexplicable, is rendered certain by the generation of coral from sea-water. That very extensive beds of calcareous matter may be produced by animals, and from their remains, is also incontestibly proved by the oolithe limestones, and by those deposits of shell marl so often found in fresh water lakes. In many such cases, in the Highlands of Scotland, it can easily be demonstrated that this is their sole origin; because we can trace the courses of the streams by which the lakes have been fed, and ascertain that they could not have carried down calcareous matter; their origin and progress lying among siliceous strata.

It must be admitted, indeed, that whatever calcareous beds may be at this moment preparing at the bottom of the ocean, as the probable germs of future strata, they will be formed like the shales and sandstones, from the ruins of the present calcareous secondary rocks; and that the operations of shell-fish will only form a part of the causes of their production. Nor need it be denied that such has been the case, to a certain degree, in former times: but that the assistance afforded by the ruins of primary calcareous rocks has been very trifling, will appear evident from a mere arithmetical comparison, which can scarcely deceive.

Every thing proves that the present secondary strata are the produce of more ancient rocks; and these must have been the continuations of those which are now the primary, as we have no reason to imagine that there has been a distinct series which has entirely vanished. The proportions of the different materials in the produce, ought, therefore, to bear a certain relation to those in the original repositories; or, if there was a difference, it should be expected to be in favour of the most yielding materials, schist and limestone. But if we examine the quantity of limestone in the primary strata, it will be found very small. What the exact proportion of limestone to the other rocks may be throughout the world is not known; but, in Scotland and England, it certainly does not amount to a thousandth part of the whole. But among the secondary strata of England, the limestones bear a far larger proportion to the siliceous and argillaceous rocks. If we were to assume only the ratio of one hundredth, it would answer the purposes of the present argument; and there is nothing unreasonable in referring the origin of the British secondary strata to the British primary rocks. In the same manner, and with the same consequences, we may refer the origin of the Apennines to the Alps. This, however, is a matter of indifference, as the general fact, taking the whole world, is indisputable.

Thus, it may fairly be inferred, that while the siliceous and argillaceous secondary strata have been formed from the ruins of more ancient rocks, a large part, at least, of the calcareous, is the produce of animals. Thus also, it must appear, that from the

operations of animals, the quantity of calcareous earth deposited in the form of mud or stone is always increasing; and that, as the secondary series far exceeds the primary in this respect, so, a third series, should one hereafter arise from the depths of the sea, will exceed the last in the proportion of its calcareous strata. It will combine the ruins of the last limestones with the spoils of the present animals; animals, of which the generations are also probably enlarging and extending in every age, in a ratio proportioned to the increase of those calcareous or soft alluvial and submarine deposits which they affect and favour. Those who, like Dr. Hutton, extend the prophetic eye of philosophy to worlds yet unborn, may also thus anticipate a constant and steady approach to that universal state of fertility which is now the pride and character of our calcareous soils.

If we now turn our views backwards to the primary rocks, we find, in the disproportion of their limestones, a confirmation of this opinion respecting the important agency of living animals in the production of calcareous strata. It has always been believed by geologists, that no animal remains existed among the primary rocks; and to avoid a breach in this hypothesis, among other reasons, the *transition* class was invented. I shall not here discuss the truth or the utility of this invention. It is sufficient to say, that the schists containing shells are consecutive to those rocks admitted to be primary, and that the only general revolution among the strata which we know, is of a later date than these. So far, therefore, as the present purpose is concerned the animal remains of the schists are primary, in as far as they are prior to the secondary strata. Nevertheless, the animal remains of the primary strata, admitting those now named, so as to give the most favourable colour to the subject, bear a disproportion to the whole of the rocks, not unlike that which the limestones do to the siliceous and argillaceous strata. This should be expected from the rarity of these animals in the ancient ocean.

It has been supposed by some geologists, that all the calcareous strata, of whatever age, were the exclusive produce of animals. That possibility is countenanced by the phenomenon of the coral

islands; though the accessory causes, arising from the decomposition of previous limestones, must be admitted, as far as regards the secondary strata. But the mere existence of primary limestones thus operating by their destruction to assist in producing new ones, is not itself a proof that these are original and independent of animal sources. The existence of animal remains in primary schists has just been mentioned; and I have elsewhere described one instance in which these occur in a limestone situated beneath gneiss. Thus far they might have contributed to the production of even the primary limestones; and if they are not more frequently found among them, causes for that are not wanting.

In the first place, primary limestones are not only comparatively rare, but geologists, having adopted the hypothetical opinion that they ought not to contain animal remains, make it a rule, invariably, to rank such instances among these *transition* series; without thinking it necessary to investigate the subject by the rigid rules of pure geological analysis; by position, and relation towards the neighbouring strata. It is further obvious, that the primary rocks have undergone great disturbance, and, in many instances, serious changes; and, even among the secondary strata, it is known that, in such cases, the animal remains are often obliterated. The fusibility of limestone has been demonstrated; and it has often been shown that many of the primary strata bear marks, scarcely to be disputed, of the action of long-continued heat. Thus it is to be expected, that their organic remains, if they ever existed, should have been obliterated; and if this has not happened in the case just quoted, of shells under gneiss, it is because the bed in which they are immediately situated, is, in fact, a quartz rock included in the limestone. If any confirmation of the plausibility of this view were required, it is found most distinctly detailed by nature in Sky, and in the Isle of Man. Where the conchiferous beds are actually converted into pure crystalline limestone by the action of the incumbent trap, it is undistinguishable from the primary rocks of the same kind, and all the shells have disappeared; while, in some parts of the gra-

dation between the stratified and fused rock, their gradual loss of form and final obliteration may be traced.

Having thus disposed of one great branch of the analogy between the primary and secondary rocks, it is necessary to see what may be inferred respecting the remainder.

The difference between shale and slate, or between the primary and secondary argillaceous schists, is often so small as to have been a source of error, even to well-trained geologists. If, when separated from their connexions, there are specimens, particularly among the oldest of the shales, which no care or practice could distinguish from the primary schists, the resemblance between the sandstones and quartz rock is often equally accurate; although, in a general sense, the latter is distinguished by its superior compactness and more predominant crystalline texture. Where it contains mica, it may be compared to the micaceous sandstones, from which it, in fact, differs only in compactness; and, when felspar is an ingredient, it is obvious that it bears an analogy to the argillaceous ones.

Here then, in primary limestone, quartz rock, and argillaceous schist, we trace an analogy, not of a very remote nature, to the secondary strata; showing that, with certain variations, from causes not difficult to comprehend, nature has repeated herself at considerable intervals of time, and has been guided by laws of great general simplicity. It remains to extend this analogy one step further; but the difficulties increase, as might be expected, at each remove.

In micaceous schist, we find an analogy to micaceous sandstone too obvious to be disputed; and whatever varieties of composition it may present, they depend on different proportions of the micaceous ingredient; the predominance of which, in particular cases, may probably be attributed to the nature of the rocks from which its materials were derived, possibly from the state of heat to which it has been exposed. Its other peculiarities are explained in a similar way. Gneiss, if we consider its materials, holds a parallel to a sandstone containing clay and mica; and here, although all analogy becomes finally very feeble, there is a

chain through the varieties of this rock which connects it with the secondary sandstones as perfectly as quartz rock is. The causes for the evanescence of this analogy consists in the posterior influence of heat. In the same manner, the action of heat has converted shale into hornblende; and thus, in the frequent alternations of gneiss and hornblende schist, we have an exact counterpart of that alternation so common between the oldest of the secondary sandstones and its concomitant shale. I need not dwell longer on a question which, interesting as it may be esteemed, is too deficient in accuracy of evidence to be a very satisfactory subject of discussion.

Although it has thus been inculcated that all the stratified rocks which are not the produce of animals, have ultimately been derived from former rocks, and probably in a series of succession the limits of which we cannot pretend to conjecture, it is still proper to remark, that there is a progressive change of character as we retreat. The limestones, it has been particularly shown, become more rare, but the argillaceous substances diminish also; so that at length, in arriving at that antiquity which, to our observation, is the highest, siliceous rocks predominate in a great degree. Thus a short-sighted philosophy might arrive at a conclusion the reverse of that formerly suggested with respect to the increase of calcareous strata, and imagine an universe once as incapable of maintaining vegetables, as it has, to all appearance, been limited in the numbers and nature of its animals; a desert of rocks and sand. But this conclusion is not justified when we take a general view of all the phenomena that geology presents. That it has been drawn, has arisen either from false theories or partial views. If the siliceous substances predominate in the more ancient parts of the series, it must be remembered that these are but the remains of rocks, of which the greater part has disappeared to form the present secondary strata; nor, in the revolutions of ages, can we decide on what has vanished, and what the state of the more ancient surface was. That it furnished a vegetable creation, and that also to a great extent, is evinced by the phenomena of coal strata, and by the enormous masses of

vegetable matter deposited through uncounted ages, and amid a series of partial revolutions of which we can scarcely form an idea.

Of the Formation of Conglomerate Rocks.

Though it has thus been shewn that, with certain rocks more or less completely furnished by animals, the secondary strata consist of the ruins of more ancient rocks, it is necessary, in treating on the origin and nature of these compounds, to bestow a few paragraphs on the conglomerate rocks, since they present some peculiarities of origin that require notice, and since they offer the most perfect evidence of the mechanical nature of the process by which the strata have been principally formed. It is indeed by tracing the gradation from the coarsest conglomerate, formed of many discordant rocks, to the finer sandstones, that we become convinced of the truth of this supposition.

As also, in nature, we can trace the analogy between the finer rocky strata and the present deposits of sand and clay from water, so in the superficial or deep-seated alluvia of a grosser kind, we find the prototypes of the present conglomerates, of the consolidated alluvia of former worlds. The nature of the evidence which these rocks afford with respect to the revolutions of the earth's surface, belongs to a subject on which I cannot here enter; but it is necessary to distinguish between those which are of a local and those which are of a more general nature.

These rocks are found, both in the ancient and recent series; and, in both, under circumstances precisely similar, if differing in extent. They are properly divisible into general and local; and it is only indeed by thus distinguishing them, that we can derive any advantage, in our reasonings on events, from the evidences they afford, or avoid the confusion to which, from incorrect observation, they have frequently given rise. As, in both the secondary and primary series, similar accidents have occurred, in the fracture, displacement, and transference of strata, it is natural to expect that the conglomerates, here called local, which have resulted from these changes, should be found in both, With re-

respect to the general ones, as they have been produced by that gradual waste of the solid rocks which now form our superficial alluvia, it is natural to expect that they should be found chiefly, and most extensively, at the great interval which separates the primary and secondary strata; and this expectation is realized by the existence of that almost universal conglomerate, the first portion of that red sandstone, which is itself the lowest and first of the secondary series.

If no revolution of so general a nature can elsewhere be traced, yet partial ones of an analogous kind are found both in the primary and secondary series; and thus, in both, there exist conglomerates which, if not universal, are still, in the sense here laid down, entitled to the name of general.

The mechanical origin of all these rocks is so obvious, that it is unnecessary to dwell on it; while it is also easy to discover that the component parts have undergone greater or less degrees of attrition, and in many cases of transportation. It is also well-known, that, with the exception of the tuff of the overlying family, they consist, in most instances, of different ingredients; and not unfrequently of a great number intermixed together.

Those which consist of many different fragments, or even of fragments of two substances, may be considered as general conglomerates. They are, in a geological sense, only modifications of the different recomposed rocks with which they are found associated; and thus, like these, they necessarily occupy extensive spaces in nature. They may thus be distinguished from the local conglomerates, by their geological position and connexions; while they may also, in a great measure, be recognised by their mineral structure; chiefly, indeed, by the attrition, whether greater or less, which the parts have undergone, and by the variety of ingredients they contain. These remarks apply principally to those conglomerates which belong to the red sandstones, of which they often form very conspicuous portions, as is universally known. Those which are connected with the overlying rocks, like the tuffs of the same division, are distinguished by such peculiarities of character as to admit of no comparison with any others

The local conglomerates, on the other hand, may be distinguished by their much greater variety as a class, and by the much more limited variety of their ingredients, sometimes consisting of only one, occasionally of two, but rarely exceeding three.

The general conglomerates are also commonly composed of materials agglutinated without an intervening cement; whereas most of the local rocks of this character consist of one or more sorts of fragments united by a third cementing substance, or by a cement composed of one of the imbedded ingredients. The local conglomerates rarely occupy any considerable space, and are often very limited; while they are always attached to some simple or compound rock, with which, in some part, they are intimately united.

As the general conglomerates form a separate and independent set of strata, the local rarely form more than one bed; and are sometimes not even found in the form of a bed, constituting a single lamina only, adhering to a parent rock, or an irregular mass, in some other way connected with it.

The general, frequently contain rounded masses, but the fragments of the local are commonly angular, or little affected by attrition. In many instances they are perfectly acute; while occasionally also, when of large size, they are found to be so little moved from their places, or separated from each other, that the imagination easily replaces the detached parts.

These rocks have been sometimes distinguished by the names of Breccia, while the others have been called pudding-stone; but as the term Breccia has also been very indiscriminately used, it is not convenient to perpetuate its application where it is necessary to be accurate.

Circumstances occasionally visible in the secondary strata, and more particularly in the calcareous, will explain the origin of these local conglomerates.

The beds of these are often found covered on the surface by their own fragments, intermixed with minuter particles of the same, or of clay. The imaginary consolidation of such a mass would form a local conglomerate; and thus it may be understood

why the angles of the fragments are so little rounded, and why the separated parts are so capable of being re-adapted. It is easy to conceive also, that the infiltration of a solution of lime would convert them into a solid rock, and that the same effects might, under other circumstances, take place from carbonate or rust of iron, or from some other of the causes that produce the consolidation of rocks.

The several conditions thus hypothetically stated, appear to have frequently existed in nature, and thus have arisen the number of local conglomerates now seen.

The fractures of the rock, and the consequent production of fragments on the surface, have probably, in all such cases, originated jointly from the ordinary causes of waste and from mechanical violence. In some instances, where the conglomerates lie between two rocks, they seem to have resulted from the motion of the parts on each other, in consequence of sudden and violent fractures, accompanied by a partial comminution of the materials.

Where one rock alone has been engaged, a conglomerate of one ingredient, united by a general cement, is the result; and this case is frequent in the calcareous rocks. When the fractures have taken place at the meeting of two strata of different rocks, or when two have been in any other mode implicated, the compound is more intricate. Thus also there are formed conglomerates of limestone and serpentine, or of limestone and argillaceous schist, or of other substances.

There is little now to be said respecting the formation of the unstratified rocks, which does not follow from the views of their origin now generally received. Of their materials, we can only know that they are those which are also found in the stratified substances, and can only conjecture, indiscriminately, that they have been formed by the fusion of some or other of these. Differences in the proportions of the several earths are the only grounds of judgment; and thus it would be inferred that granite was the produce of gneiss, micaceous schist, quartz rock, and ultimately of argillaceous sandstones, and that the ordinary traps

were the produce chiefly of the argillaceous substances, slate or shale. The more particular evidences in confirmation of this opinion, belong to the histories of trap and granite, on which I cannot here enter.

Of Transitions among Rocks.

The last question respecting rocks that appears to require examination, relates to the transitions, real or imaginary, that take place between different kinds of rocks. Being formed, as we have seen, of so few substances, and possessing so many analogies among each other, such transitions ought to be expected. That they exist, is no reason for an hypothesis which has been maintained on this subject.

Because there is a gradation of a certain kind among gneiss, micaceous schist, and quartz rock, and because it is possible, by selecting particular specimens, to make that transition still more extensive, it has been argued that all these rocks originated at one time, from a common solution, and are, therefore, the results of a continued crystallization from a fluid gradually varying. Geologists who have chosen to maintain this doctrine have certainly derived from it great convenience; inasmuch as they have dispensed with the labour of investigating the differences of these rocks, or describing their characters and connexions. I know not what advantages are to be gained by thus restoring geology to its original chaos; and as the question of watery crystallization has been sufficiently considered by numerous authors, the reader and writer may both equally be saved the trouble of an uunecessary discussion. Such transitions as do actually occur, may easily be accounted for in various ways. In the older strata they may arise from proximity of position in rocks that have been in a state of semi-fusion, and that were formed of similar materials. Thus they are common between gneiss, micaceous schist, and quartz rock, accordingly as these approximate. Thus, by intermixture, occasional transitions may also happen between coarse argillaceous schist and quartz rock, or between the fine and gneiss. But these are rare and easily explained; nor is there any tran-

sition from limestone to any other rock. In the newer strata, it is equally easy to understand how this must happen, from the irregular succession of so small a number of materials; and how some uncertainty of composition must often take place at the point of change between different deposits. The subject indeed scarce seems worthy of a general discussion, and the particular transitions comprise a subject not within the limits of this paper. It is to be proved that the imaginary value attached to these transitions has arisen from the practice, far too general, of deducing conclusions respecting the order of nature, from that made by a mineralogist in his cabinet. Undoubtedly, a rich cabinet may be made to produce every transition which the most arduous theorist could desire; but he will have far mistaken the real objects of his geological pursuits, who shall make his drawer the type of nature.

J. M'CULLOCH.

ART. V. *Supplementary Remarks to a former Paper on Light and Heat. By* BADEN POWELL, M.A., F.R.S.

(1.) In a paper on light and heat from terrestrial sources, inserted in the last Number of the *Journal of Science*, &c. (§. 8, and 26,) I alluded to experiments on the heating power of the light from incandescent metal, but without particularly stating any such results. As the fact is perhaps somewhat remarkable, it may not be improper here to mention, that I have always found the effect exhibited to the amount of a rise of 10° or more in thirty seconds, with Leslie's photometer, from a ball of iron two inches diameter, heated to the brightest degree in a common fire. But for the sake of those who may wish to repeat the experiment, it may be necessary to remark, that several precautions must be taken. The instrument, if of what is termed the portable kind, that is, having its bulbs in the same vertical line, and the stem of the

upper passing in contact with the lower, must not be used without its case, and should also be screened by glass, for if it be exposed, the heating power of the light no longer acts upon it alone, but the effect is increased from other causes. If the bulb be *painted* black, this increase is in part owing to the greater *absorptive* power of the coated, than that of the plain bulb, for the *simple heat* admitted to it by the removal of the case and screen. But partly also it is owing to another cause. The stem which passes in contact with the lower bulb being of thicker glass, is longer in acquiring heat than the lower bulb is; and therefore will initially cool it, and thus increase the apparent effect on the other bulb. Such an increase is owing to the presence of simple heat, and could never be produced by light.

(2) When the instrument employed is of the "stationary" kind, that is, having its bulbs at equal heights, and (as in the one I used) having one bulb blown of black enamel, there are other considerations to be attended to. In fact, I have found that this instrument, when exposed without case or screen, not only exhibits no effect upon the black bulb, but shews a considerable depression on the side of the plain bulb; this only continues for about two minutes, when the other begins to be affected. The same thing takes place, perhaps in a less degree, if the case be used, and become considerably heated; the same also occurs if the instrument be used without its case, but screened by a piece of glass, the glass becoming heated. When both the case and a glass screen are employed, this effect is not produced, but the instrument remains stationary, or nearly so; sometimes a trifling effect seems to be produced on the black bulb. But if a second screen be used also, I have always found an effect of about two degrees, or rather more, in one minute on the black bulb. The outer screen becomes hot, the second very little so, and the case remains quite cool; the screens were from 1-10th to 1-8th inch in thickness.

(3.) It is impossible to ascribe the effect through the screens to any thing but the heating power of light; but it is possible that it is less than what ought to be the case, if we suppose the opposite

effect above described not to have been sufficiently excluded. The larger size of the bulb may have partly caused the effect to have been so much less with this than with the smaller instrument, the diameters of the bulbs being respectively 0.35 and 0.6 inch, besides the additional causes in operation on the small instrument tending to increase the effect before noticed.

(4.) The peculiar effects above described on the larger instrument, appear to be due to some greater action on the plain bulb; in proportion as this interfering cause is excluded, the light from incandescent metal is capable of displaying its heating effect, and the action on the plain bulb cannot be ascribed to light, but must depend upon some peculiar action of simple radiant heat. It is evident that if there were any thing tending to make the simple heat act more on the plain bulb, its effects would be displayed when the screen was removed. This might be the case from the circumstance that the plain bulb was of rather smaller diameter, and the glass probably thinner than the black enamel. It might also be a question, whether a greater apparent effect on the plain bulb might not be in part occasioned by the greater expansion of the black glass.

Thus, upon the whole, we may regard these apparent anomalies as tending to establish still more firmly the essential distinction between these two heating agents at first pointed out.

(5.) To those who have been led to attend accurately to the nature of the instrument in question, it will be needless to make any further observations on the several causes of inaccuracy affecting its indications, which have been recently pointed out by several experimenters. The use here made of it, however, is not of such a kind that the results would be materially influenced by any such inaccuracy, and the main facts have been established upon investigations conducted in a very different manner.

(6.) The following are a few of the experimental results above referred to:—

Small Photometer; incandescent iron.

Rise in the first thirty seconds.			
Expr.	No case.	Case.	Case and 2 screens.
1	15	8	8
2	11	11	9

See above (1.)

Large photometer: glass case: two screens of plate-glass replaced after each experiment, the nearest close to, but not touching the case; the second at about one-quarter from first: incandescent iron ball, four inches distant.

Secds.	Expr. 1	2.	3.
0	25	25	25
30	26	26	27
60	27	27	28

After experiment, outer screen hot, inner very little so, case cool.

Secs.	Screen no case.	Expos'd
0	25	25
30	22	20

After experiment screen heated.

Flame distant 1.5 inch.

Min.		Expr. 1.	2.
0	screened	25	25
1	exposed	30	28
2		29	28

See above (2.)

Art. V. *Observations on the State of Education in Ireland.* *By* George Harvey, Esq., F.R.S., L. and E.

[Communicated by the Author.]

Through the kindness of Mr. Rickman, I received in November last, the Population Returns for Ireland for the year 1821, and from its being the first authentic enumeration of the Irish people, and from its having been carried on under the most favourable auspices, and, according to Mr. Shaw Mason, with every precaution for ensuring accuracy on the parts of the enumerators *, the results cannot but be highly interesting to the philanthropist, the philosopher, and the politician.

It is not my intention, on the present occasion, to enter on the

* "The enumerators were called upon to make a *preliminary return*, according to a form transmitted for the purpose, specifying the names and number of the parishes, townlands, or other subdivisions of the district to which they had been appointed; the names and addresses of the clergymen of every religious persuasion, actually resident within their respective districts, as also the names and addresses of schoolmasters of every description resident therein, with the names of the townlands, &c., on which their schools were kept. In consequence of this application, several who had been appointed voluntarily resigned, from a consciousness of their own inadequacy; some were found to be incompetent, and so reported to the bench of magistrates, whereupon their places were supplied by others deemed more capable of furnishing the information, on their producing proof of possessing the qualifications required. Other beneficial results also accrued from this process. Various points relative to the position, connexion, names, or other local circumstances of the country, were explained, and errors rectified. Some of the returns also contained much additional information of great value in the progress of the inquiry. The returns of the names of the clergy and of the schoolmasters opened a wide field of communication, which proved essential towards ultimate success.

" After it had been thus ascertained that every district was supplied with an enumerator qualified to make a satisfactory return, a copy of printed instructions was forwarded to each, detailing the steps to be taken by him in his operations under the act. These instructions were framed on the principle, that nothing should be required from the enumerators *but matter of fact*, excluding any thing depending solely on opinion, or on deductions to be formed from the facts so collected; as also, that as much additional information should be collected as could be done, without an undue interference with the time or attention requisite for attaining the main object of the census."

consideration of the deeply-interesting question of the rapid increase of the Irish population, and of the many remarkable results which the present returns disclose; but to offer for the consideration of your intelligent readers, a few observations on the state of education in Ireland, drawn from the number of pupils of both sexes in the several counties. I feel it necessary, however, to confess, notwithstanding the declaration of the learned editor of the Returns *, that I entertain some fears that this part of his labours does not entirely merit our confidence; that either the enumerators did not in this case perform faithfully their duty, notwithstanding the laudable care displayed by the Irish government in selecting proper persons for that purpose; or that the returns are only calculated to increase that regret which most of us have been accustomed to feel, when reflecting on the low and degraded state of that generous people. At all events, the discussion of the question can do no harm, for if the returns which relate to the schools *are correct*, it ought to quicken our diligence and zeal in favour of so low and degraded a people; and if *incorrect*, that more earnest endeavours should be exerted at the next enumeration of the people, to obtain more accurate and faithful returns; nor shall I regret, in case the latter supposition should be true, the labour the computation of the succeeding tables has cost me, if it should lead to more accurate and faithful returns for the future †.

* This declaration is to the following effect: (See page 16 of Preliminary Observations):—

"The instructions given to the enumerators on this point (schools and pupils) were precise and particular; they were to inform themselves not only of the situation of every school within their district, but also of the names of the teachers, the number of pupils, both male and female, and the nature of the endowment (if any) by which they were maintained. These particulars were deemed of sufficient importance to justify their admission into the abstract prepared for Parliament, in which they appear in columns, containing a statement of the number of children, both male and female, actually receiving public instruction at the time of taking the census. When the schools received the whole or part of their support from eleemosynary sources, the particulars, both as to the nature of the foundation and the names of the contributors, have also been given in the column of observations."

† Since this paper was drawn up, I have received the first Number of the

On the supposition that the returns are correct, an estimate may be made of the state of education, by comparing the pupils enumerated, either with the children actually existing from 5 to 10 years of age, or from 5 to 15, the latter being probably the limits between which the majority of the pupils must be found. In this way the intensity of education may be discovered, and those districts made known where its blessings are experienced in the highest degree, or where ignorance, superstition, and error, most abound.

The first of the following tables has been deduced from the Population Returns, for the purpose of making a comparative estimate of the different degrees in which male and female education prevails, and from which it will be perceived that low and degraded as is the state of the males the condition of the females is still more to be deplored. The table exhibits for both sexes the relation between the pupils found in each county, and the total population of all ages, and which mode of comparison was necessarily adopted in the present case, on account of the distinction of sex having been unfortunately omitted in the classification of the ages. The counties are arranged in the order adopted in the Population Returns; and it will be immediately seen by the numerical results, what rank each county holds in the scale of moral and intellectual improvement.

Dublin Philosophical Journal, containing an interesting paper on the population of Ireland, and in which the author observes, "we regret that the number of pupils at the schools were inserted. They are obviously entirely void of accuracy, and were any conclusions to be drawn from them, they must be entirely fallacious." This remark, however, is too sweeping in its consequences. The returns *may* be correct, and our ideas of their inaccuracy erroneous; at all events, calling the public attention to the question can do no harm.

LEINSTER.			MUNSTER.			ULSTER.			CONNAUGHT.		
COUNTIES.	Relation between the Male Pupils and the Total Male Population of all ages	Relation between the Female Pupils and the Total Female Population of all ages.	COUNTIES.	Relation between the Male Pupils and the Total Male Population of all ages	Relation between the Female Pupils and the Total Female Population of all ages	COUNTIES.	Relation between the Male Pupils and the Total Male Population of all ages	Relation between the Female Pupils and the Total Female Population of all ages	COUNTIES	Relation between the Male Pupils and the Total Male Population of all ages.	Relation between the Female Pupils and the Total Female Population of all ages.
Carlow .	1 in 10	1 in 14	Clare .	1 in 12	1 in 27	Antrim .	1 in 9	1 in 19	Galway .	1 in 20	1 in 49
Drogheda town	1 in 12	1 in 22	Cork .	1 in 11	1 in 25	Armagh .	1 in 11	1 in 26	Galway town	1 in 12	1 in 17
Dublin . .	1 in 12	1 in 23	Cork city	1 in 8	1 in 14	Carrickfergus town .	1 in 9	1 in 12	Leitrim .	1 in 17	1 in 39
Dublin city	1 in 10	1 in 21	Kerry .	1 in 11	1 in 30	Cavan .	1 in 16	1 in 36	Mayo .	1 in 24	1 in 46
Kildare . .	1 in 13	1 in 20	Limerick	1 in 8	1 in 21	Donegal .	1 in 20	1 in 35	Roscommon	1 in 15	1 in 32
Kilkenny .	1 in 9	1 in 23	Limerick cy.	1 in 9	1 in 12	Down . .	1 in 12	1 in 26	Sligo .	1 in 12	1 in 24
Kilkenny city	1 in 6	1 in 14	Tipperary	1 in 12	1 in 26	Fermanagh	1 in 16	1 in 29			
King's county	1 in 12	1 in 25	Waterford	1 in 14	1 in 41	Londonderry	1 in 20	1 in 48			
Longford .	1 in 11	1 in 21	Waterford cy	1 in 9	1 in 28	Monaghan .	1 in 19	1 in 33			
Louth . .	1 in 18	1 in 44				Tyrone .	1 in 15	1 in 33			
Meath . .	1 in 16	1 in 31									
Queen's county	1 in 14	1 in 34									
Westmeath .	1 in 13	1 in 27									
Wexford .	1 in 10	1 in 23									
Wicklow .	1 in 10	2 in 18									

Similar results for the four great divisions of the country, Leinster, Munster, Ulster, and Connaught, and also for the total population of Ireland, are contained in the next table.

PROVINCES.	Relation between the Male Pupils and the Total Male Population of all ages.	Relation between the Female Pupils and the Total Male Population of all ages.
Leinster . .	1 in 11	1 in 23
Munster . .	1 in 11	1 in 24
Ulster . .	1 in 14	1 in 29
Connaught .	1 in 18	1 in 37
Total Population of Ireland .	1 in 13	1 in 27

The first impression derived from a review of the preceding tables, is the great disparity between the numerical results obtained for the two sexes, in *all* the greater divisions of the country, the inequality arising from the great neglect of female education. The nearest approach to equality is in the towns, but even there the difference is considerable. In the city of Dublin, for example, the education of females is represented by the ratio of *one* in *twenty-one*, whereas for the males the relation is indicated by *one* in *ten*, not that I am disposed to consider the latter as indicating any thing like a favourable instance of what the education of a people ought to be. In a great majority of instances, indeed, the education of males exceeds that of females in the ratio of *two* to *one*, and in some cases nearly *three* to *one*. It may also be observed, that the education of males for the whole of Ireland exists in a maximum degree in the city of Kilkenny, the relation being *one* in *six*; but for females it attains the greatest state in the city of Limerick, the ratio being *one* in *twelve*. In Leinster the education of males varies from *one* in *six* to *one* in *eighteen*, the minimum state being found in Louth. In Munster it fluctuates be-

tween *one* in *eight*, and *one* in *fourteen*, the maximum being found either in the county of Limerick or the city of Cork, and the minimum in the county of Waterford. In Ulster, the intensity of education is the *greatest* among the male sex in Antrim and the city of Carrickfergus, the relation being *one* in *nine*, and the *least* in Donegal and Londonderry, the ratio being *one* in *twenty*. In Connaught, education abounds the most in the town of Galway and in the county of Sligo, and the least in Mayo, the former relation being *one* in *twelve*, and the latter *one* in *twenty-four*. In Leinster, the education of females varies from *one* in *fourteen*, to *one* in *forty-four*, the former relation being found in Carlow and the city of Kilkenny, and the latter in the county of Louth. *Hence it appears that, both for males and females, the state of education is the lowest in the county of Louth.* In the province of Munster, it fluctuates from *one* in *twelve*, to *one* in *forty-one*, the former relation being found in the city of Limerick, and the latter in the county of Waterford. For Ulster, the extremes for females are *one* in *twelve*, and *one* in *forty-eight*, the former existing in the town of Carrickfergus, and the latter in the shire of Londonderry. In the province of Connaught, the maximum relation is found in the town of Galway, the ratio being *one* in *seventeen*, and the minimum in the county of the same name, the relation being *one* in *forty-nine*.

It appears, therefore, that the state of education in Ireland bears no fixed and definite relation to the entire population; and we shall find, from a subsequent table, that anomalies equally remarkable exist, when the inquiry is confined to adults alone. The last of the preceding tables exhibits, indeed, in a small compass the unequal degree in which its blessings abound in the different provinces, and also the very inferior degree in which the female mind is instructed and improved. In the whole of Leinster and Munster, the education of the male is *one* in *eleven*, and of the female in the former province only *one* in *twenty-three*, and in the latter only *one* in *twenty-four*. In Connaught, the intellectual state of the males may be represented by *one* in *eighteen*, and of the females by *one* in *thirty-seven*; so that in the four provinces

the intensity of education for the males varies from *one* in *eleven*, to *one* in *eighteen*, and for the females from *one* in *twenty-three*, to *one* in *thirty-seven*. For the whole population the ratio for the males is *one* in *thirteen*, and for the females, *one* in *twenty-seven*, the former relation exceeding the latter in a greater ratio than two to one. Such are the unequal degrees in which the inestimable blessings of education prevail in this much neglected country!

But it may possibly be remarked, that the comparison of the *number of persons educated* with the *total population* of both sexes, and on which the preceding investigation has been founded, is not an accurate and proper mode of considering the question. This will be admitted, and the only reason for its adoption was the impossibility of obtaining, by any other means, the relative states of male and female education,§ in the different districts of the kingdom, a point most interesting to determine, since nothing indicates more clearly the degree in which civilization prevails in any country, than the state and condition of the female.

In the following table, therefore, the persons educated are, in the first place, compared with the total population between the ages of five and ten; and secondly, with the total population between the ages of five and fifteen. These methods were adopted on account of the population returns readily affording the number of persons actually existing at those ages, at the time of the census. The counties and cities are not arranged in the order adopted in the preceding table, in conformity to the returns, but according to the *numerical* values of the results indicating the state of education.

LEINSTER.

COUNTIES.	Relation of Total Male and Female Pupils, to Total Male and Female Population between 5 and 10.	COUNTIES.	Relation of Total Male and Female Pupils, to Total Male and Female Population between 5 and 15.
Kilkenny city .	1 to 1.0	Kilkenny city .	1 to 2.0
Dublin city .	1 to 1.3	Dublin city .	1 to 2.6
Carlow . .	1 to 1.6	Carlow . .	1 to 3.0
Wicklow .	1 to 1.8	Wicklow .	1 to 3.4
Drogheda town	1 to 1.9	Kilkenny .	1 to 3.5
Dublin . .	1 to 1.9	Dublin .	1 to 3 6
Kilkenny .	1 to 1.9	Drogheda town .	1 to 3.6
Wexford .	1 to 1.9	Wexford .	1 to 3.6
Longford .	1 to 2.0	Longford .	1 to 3.9
Kildare .	1 to 2.1	Kildare .	1 to 4.0
King's county .	1 to 2.2	King's county .	1 to 4.2
Westmeath .	1 to 2.4	Westmeath .	1 to 4.5
Queen's county	1 to 2.7	Queen's county .	1 to 5.1
Meath . .	1 to 2.8	Meath .	1 to 5.3
Louth . .	1 to 3.3	Louth . .	1 to 6.3

ULSTER.

Carrickfergus town	1 to 1.2	Carrickfergus town	1 to 2.5
Antrim .	1 to 1.6	Antrim . .	1 to 3.2
Armagh .	1 to 2.0	Armagh .	1 to 4.0
Down . .	1 to 2.1	Down .	1 to 4.1
Tyrone . .	1 to 2.7	Tyrone .	1 to 5.3
Fermanagh .	1 to 2.8	Fermanagh .	1 to 5.4
Cavan . .	1 to 3.1	Cavan . .	1 to 6.0
Monaghan .	1 to 3.1	Monaghan .	1 to 6.2
Donegal .	1 to 3.6	Donegal .	1 to 6.9
Londonderry .	1 to 3 7	Londonderry .	1 to 7.4

MUNSTER.

COUNTIES.	Relation of Total Male and Female Pupils, to Total Male and Female Population between 5 and 10.	COUNTIES.	Relation of Total Male and Female Pupils, to Total Male and Female Population between 5 and 15
Cork city .	1 to 1.2	Cork City .	1 to 2.3
Limerick city	1 to 1.3	Limerick city .	1 to 2.5
Limerick .	1 to 1.6	Limerick .	1 to 3.1
Waterford city	1 to 1.7	Waterford city .	1 to 3.2
Cork .	1 to 2.2	Cork .	1 to 4.1
Kerry .	1 to 2.3	Kerry .	1 to 4.2
Tipperary .	2 to 2.4	Tipperary .	1 to 4.5
Clare .	1 to 2.6	Clare .	1 to 4.7
Waterford .	1 to 2.9	Waterford .	1 to 5.3

CONNAUGHT.

Galway town	1 to 1.7	Galway town .	1 to 3.2
Sligo .	1 to 2.3	Sligo . .	1 to 4.2
Roscommon .	1 to 2.9	Roscommon .	1 to 5.3
Leitrim .	1 to 3.6	Leitrim .	1 to 6.8
Galway .	1 to 4.0	Galway .	1 to 7.4
Mayo .	1 to 4.5	Mayo .	1 to 8.3

The first thing thing to be remarked with respect to the last table is, the *order* of the places selected for observation, it being the *same* in all the provinces for the results deduced for the population between the ages of five and ten, as for those between five and fifteen, with the exception of a trifling anomaly in Leinster, relating to the town of Drogheda and the county of Kilkenny. This variation, however, from the law of uniformity is so very inconsiderable, that it can scarcely be regarded as an interruption of it; and the intensity of education, as deduced for the total population between the ages of five and ten, may therefore be regarded as proportional to that between five and fifteen; the relation being in general terms that of two to one.

In the four provinces the maximum and minimum intensities, or the places in which education abounds in the greatest and least degrees, may be exhibited as follows:

PROVINCE.	Maximum of Education.	Minimum of Education.
Leinster	Kilkenny city	Louth
Munster	Cork city	Waterford
Ulster	Carrickfergus town	Londonderry
Connaught	Galway town	Mayo

and from which it appears, as before remarked, that education is most generally diffused in towns, and in the least degree over the wide surfaces of counties. Its intensity, however, in towns, is not uniform, it varying in the population between 5 and 15, from 1 in 2, to 1 in 3.6. If we consider the blessings of education confined to the population between the ages of 5 and 10, the city of Kilkenny will present the interesting example of the *whole* of its children being educated; but if we embrace all the adults from 5 to 15, then only *half* the children enjoy the advantages of instruction. In the county of Mayo it will also be found that only 2 in 9 are educated, according to the former of these suppositions, and only 3 in

25 by the latter. So likewise only 3 in 11 will be found in the first class for Londonderry, and 3 in 22 for the second. If indeed we direct our attention exclusively to the numbers contained in the second column for each province, a most humiliating picture is presented of the state of education, no town or county presenting an example of the whole of its adult population receiving the advantages of education.

I would again observe, in conclusion, that the preceding remarks have been formed on the supposition that the *returns are correct*, and I have therefore abstained from making any observations but what are strictly warranted by the tables themselves, and the sources from which they were derived. I have refrained also from making any allusion to the state of religion in the different counties, from the full and perfect conviction that comparisons of the kind only tend to foster and perpetuate a spirit diametrically opposed to the mild and beneficent principles of Christianity.

Plymouth, January 3, 1825.

Art. VII.—*An Account of the Eruption of Mount Etna, of the* 27*th May*, 1819.

[Extracted from the Journal of Signor M. Gemmellaro.]

During the first four months of the year 1819, the mountain was remarkably quiet, and no other phenomena were remarked in the crater than the occasional evolution of white vapours. It remained thus tranquil until the morning of the 27th of May, when it suddenly sent forth copious volumes of smoke, and the whole mountain trembled. Three large mouths or caverns opened very near those which were formed in the eruption of 1811, at the south-east foot of the high *bicorne*, in the lava called *del filosofo*, formed in 1792, about half a mile distant from my cottage*. Flames, and red-hot cinders and sand, amidst dense smoke, soon began to burst into the air from these mouths, with an inconceivable noise, and the dust, transported by the wind, covered

* See plate IV., fig. 2.

and dried up the grass, and blackened the roofs of the houses from Nicolosi to Aci Reale, *&c.* A few minutes afterwards, another mouth opened below, under the ridge of the large valley del Bove (which the mountaineers call Trifoglietto), whence issued forth flames and smoke; and finally, a fifth, lower still, in the Rocca di Giannicola, above that of Corvo, from whence issued a torrent of lava, which spread itself with great velocity over that valley. Amidst so many crashes, and such great confusion, and the atmosphere so dense and black around the mountain, the inhabitants of the villages were almost all in motion, and many affirm, that about mid-day, various shocks of earthquakes were felt in Nicolosi, in Viagrande, and also in Catania.

May 28*th.*—The explosions, which shook the mountain, continued the whole of this day, and the course of the lava was so rapid, that in less than twenty-four hours it arrived at Zappinello, which is two miles distant from its source. In the evening, the lightning, amidst the column of flame and smoke, recalled to mind "l'enorme e rigoglioso pino," so celebrated in Vesuvius. It is clearly seen, that the liquid lava flows by a superficial channel from the centre of the great crater, although neither smoke or flame issue from it.

29*th.*—The explosions of the higher mouths continued, and the course of the lava proceeded with nearly the same rapidity, and extended almost two miles farther, arriving at Carazo in the fields. The inhabitants of Zafarano begin to be in fear for their farms and village. The high part of Etna, as the *Piano del Lago*, the *Torre del Filosofo*, my house, and the great ditch della Cisterna, particularly, are cracked with long gaps, which also traverse the hill upon which stands the Torre del Filosofo. The three original mouths are become two, that is, one of them remains as it was, and the other two are united into one. The cinders that have fallen everywhere around the mouths have formed a very curious wreath, composed of various colours of sulphur and of muriate of ammonia.

30*th.*—The body of the lava is much retarded to-day, therefore it spreads itself in various directions.

31st.—The lava is losing its fluidity and motion; and it appears that it will stop near La Mandra di Pantano. The explosions still continue strong and noisy, but the chief crater sends forth very little smoke.

June 1st.—The lava stops at the Mandra di Pantano, and puts an end to the fear entertained at Zafarano, but it still continues to run, from time to time, in the Contrada di Giannicola.

2nd.—The chief crater is still without smoke.

From the *3rd* to the *8th.*—The lava resumes its course, still amassing itself, the preceding body forming a bed for the succeeding one. It increases in quantity, and the explosions and detonations increase equally, in an extraordinary manner.

9th.—The detonations are stronger than at first.

10th.—The detonations to-day are heard seldomer, but are louder, and accompanied by tremblings.

11th.—The detonations are as strong as at first, but instantaneous, and exactly similar to the firing of a large cannon.

12th and *13th.*—Majestic globes of smoke, more frequent, and filled with sand, issue from the chief of the high mouths, and reach, in fine dust, to Catania. At 4 P. M., the superior mouth, or that near the Torre del Filosofo, vomits portions of lava, and the four inferior apertures also send forth lava, which forms two torrents. Both run down the valley del Bue, in length about 240 feet, carrying desolation before them. A mass of debris now covers all the apertures from which the great smoke arose, from the running subterraneous fluid. The aspect of these ruins is awful; it displays caverns of immense depth, masses suspended, and rocks of ancient lava broken and destroyed.

14th.—The streams of lava from the superior mouth are more copious, a great furrow is open amongst the rubbish of the *Ciglioné*, and from this proceeds smoke, explosions of stones, some of ancient lava not decomposed, and others of new lava; meanwhile, the lava runs from the higher, the middle, and the lower mouths, increasing in quantity by the masses rolling over each other, and reaching as far as the Contrada di Zappinelli.

15*th.*—The streams from each mouth continue to be copious, and cover all the neighbourhood.

16*th.*—The detonations are lessened, and without shocks, but the globes of fire and smoke are in greater abundance.

17*th.*—The stream of the lava is augmented; the globes of sand are become larger.

18*th.*—Loud roarings are heard continually.

19*th.*—Lava does not issue from the fissures of the Ciglioné before mentioned, but only from the ancient apertures, and with little roaring. The high crater smokes a little to-day, the smoke being whitish.

20*th.*—The crater becomes quiet, that is, there is no smoke, and the lava runs as before, but with less noise.

21*st.*—The chief crater smokes at intervals; this is, perhaps, caused by the hail-storm, and by the west wind, which has blown a few hours. The original mouth remains bounded by a small circular mountain, which is formed by the erupted cinders and sand; the bottom of this gulf encloses three apertures or mouths, from the first of which smoke and stones are hurled into the air, with great noise; from the second, smoke and cinders; and from the third, smoke and vapour.

23*d.*—From the foot of the above-mentioned small mountain, a stream of lava runs as before; the last mouth is divided by a small intersection into two. The lava, in the mean time, issues as before, and great stones are also, at times, projected, with a violent crackling noise. The torrent appears to take the direction of the valley di Calanna, and blocking up the pass by which it would reach the district of Giannicola, it overflows that valley.

24*th.*—The detonations at the mouths continue, with more force, and without interruption. The lava which overflows the valley di Calanna presents a most singular and surprising sight; it hurls itself down an almost perpendicular fall, while part of it being hardened in the descent, and then falling to the bottom, makes an inconceivable crash and noise; and precipitating itself so rapidly, it carries with it the soft part of the hill, and forms an immense column of dust, more remarkable than the

smoke vomited from the different mouths at the time of the eruption. This circumstance caused many who observed it from a distance to believe that a new aperture was formed in that place. The original high mouth, or truncated cone, does not send forth any more cinders or smoke, neither are the roarings heard so strongly as at first. The lava occupies little space in the valley di Calanna; it is exactly between the Acqua Grande, and Acqua Rossa. The smoke, mixed with dust, ascends, as yesterday.

26th.—The lava does not advance more than sixteen feet an hour; at 5 P. M., smoke is discharged from the higher mouth, mixed with the finest sand, which falls at a great distance.

27th.—The lava runs to day much quicker in the above-mentioned valley, having increased in quantity, by the junction of several small streams. The smoke, mixed with sand, continues, and is driven, by the force of the wind, beyond Cape Pachino. It is singular, that no smoke appears from the great crater.

28th.—The cone is swallowed up on the north-west side, opposite the high Bicorne, so that the explosions of scoria come from the enlarged mouth, and the smoke and sand from the broken cone itself. Towards five in the morning—the west wind forces the smoke and sand on the other side, so that we are thus liberated from these dismal and severe showers. All the vicinity of the original higher mouth, which had been, until to-day, covered with a quantity of scoria and fragments of stones, upon which it was not possible to walk without danger, is now covered by the sand that has fallen there, in such a manner, that it remains level, like the surface of the Piano del Lago. The lava does not make so much progress in the valley di Calanna.

30th, and *July 1st.*—The lava in the above-mentioned valley has ceased to advance; in the night, at ten minutes before twelve, an earthquake was sensibly felt, but more in Catania than upon Etna. The motion seemed to oscillate from south to north.

2nd.—The lava within the valley di Calanna begins to cool, and the highest mouth roars but seldom.

3rd and *4th.*—The small hillock of the truncated cone, from

being broken, as it was on the 28th ultimo, is already formed anew, and is regularly circular; it emits sand, scoria, and smoke, but all at the same time, and not at intervals, as formerly. The lava runs within the valley del Trifoglietto, in the same quantity as before; in the Contrada di Giannicola it widens, and expands towards the lands of the Solifizio; within Calanna, it has ceased to run.

5th.—The cone sends out smoke, fire, and sand, without noise.

6th and *7th.*—The lava begins again to flow a little in the valley di Calanna, and it is feared that it may bury the source of the Acqua Rossa. On the other side, it threatens the Contrada del Cerrazzo.

8th.—From the cone, the smoke issues from two neighbouring throats or apertures, with more noise than before.

9th and *10th.*—The cone smokes and roars terribly.

11th.—The cone emits smoke with its usual force, and its northern throat is considerably enlarged, and is that from which issue immense columns of smoke, with scoriæ, and rumbling noises.

12th and *13th.*—The highest crater begins to be quiet; the cone diminishes its northern throat, and emits little smoke; the lava appears to take the direction of Il Cerrazzo, and passes that of 1811.

14th.—The roarings are more terrific and deafening.

15th, *16th*, and *17th.*—The interior of the cone remains divided on the southern side, and vomits smoke, with the usual frightful noise; and in the northern half, it would be almost choked up, if it were not for small holes which exhale smoke, and through which the liquid and red-hot matter can be observed at the bottom. The portion of the *Ciglionć* of the valley del Bue, which fell in on the 13th of June, is now sunk somewhat more. The lava continues to run in quantity, and with great velocity, and ramifies within the valley del Bue.

18th to the *27th.*—The lava runs from the cone, but without noise.

28th to the *31st Idem*, *August 1st.*—The lava is now suddenly stopped.

NOTE.—It will not, perhaps, be tedious to the reader, if I repeat what I have before communicated to many of the philosophers of Europe, upon the subject of the Electricity upon Etna:—On the 2nd of June, 1814, before mid-day, two travellers were returning from the mountain, guided by Vicenzo Carbonaro, one of my guides, from Nicolosi. They had arrived in the Piano del Lago, where, expecting a hail-storm, they quickened their pace, walking upon the frozen snow. Carbonaro was the most advanced of the party, he felt his hair stand an end, his forehead and the skin of his face felt benumbed, and he heard a hissing noise. He took off his cap, and his hair became more bristled, and the whistling noise more powerful. The traveller nearest to Carbonaro also heard a humming sound, and asked the guide what it was; he could not give him any reason for it, and he stopped, supposing he was dizzy. In the mean time, they approached each other, and were pleased with the magic sound. The traveller turned to call his companion, who was at a like distance off, and made a sign to him with his hand; the hand, when raised, produced a much stronger sound; so much so, that moving the fingers singularly modulated it. The traveller approached, and heard the sound produced by the head and body of his companion. He wished to imitate his companion, but not having entered into the current of electric air, his repeated attempts prodnced no sound. Finally, the three persons having joined, they experienced great pleasure, as with moving their fingers, they produced the above extraordinary effect. In the mean time, the hail-storm fell on them, and being rather curious than erudite, they resolved to prosecute their journey downwards, without caring to make further investigations. Scarcely had they gone a few paces, going out of the electric air, than the sounds ceased, and Carbonaro was relieved from his apprehensions.

Description of the Plates.

PLATE III.—A COMPARATIVE CHART OF THE PERPENDICULAR HEIGHT OF ETNA.

Fig. 1. Gemmellaro's cottage.
2. Philosopher's Tower.
3. Plain of the Lake.
4. Rock of the Lake.
5. Tempa del Barile.
6. Montagnuola.
7. Uninvestigated region.
8. Mount Castellacci.
9. Termination of the woody region.
10. English Grotto.
11. Casa delli Rinazzi.
12. Monti Rossi.
13. Nicolosi.
14. Monte Piliere.
15. Terre di Grifo.
16. Mascaleia.
17. Plachi.
18. Lava of 1669.
19. Fasaro.
20. Catania.

Plate IV.—EASTERN SIDE OF ETNA, SHEWING THE ERUPTION OF 1819.

Fig. 1. The Cone of the Crater.
2. Gemmellaro's Cottage.
3. The Philosopher's Tower.
4. Montagnuola.
5. Serre di Soliflzio.
6. Monte Zoccolaro.
7. Serre di Calanna.
8. Zappinelli.
9. The plain.
10. The valley of Calanna.
11. Monte Calanna.
12. Ancient Lava.
13. Village of Zaffarana.
14. Fiuri di Cosimo.
15. Mandra di Pantano.
16. Cerrazzo.
17. Bocca di Musarra.
18. Lava of 1811.
19. Monte Finocchio.
20. Monte St. Simone.
21. Valle del Bove.
22. Rocca di Giannicola.
23. Trifoglietto.
24. Upper mouth of the Eruption.
25. Course of the Lava.
26. Ditto, terminating in the valley of Calanna.

Plate V.

Fig. 1, Crater of Etna, as it appeared in the years 1804 and 1805.
2. Ditto, from 1805 to 1809.
3. Ditto, from 1809 to 1816.
4. Ditto, from 1816 to 1824.

Art. VIII. *On the Theory of the Wedge.—To* Dr. Young.

[To the Editor of the *Quarterly Journal of Science*, &c.]

Sir,

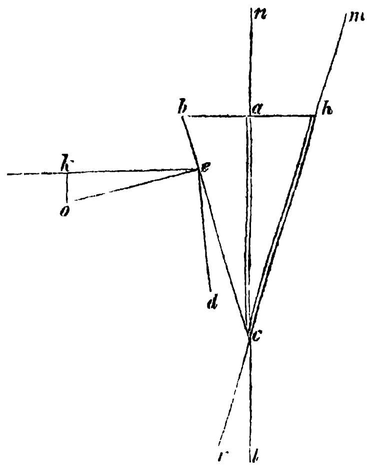
I take the liberty to request that you will allow me a small space in your *Journal*, to point out what I conceive to be an error, which has somehow crept into several of our best elementary works on Mechanics.

The error I allude to is in the theory of the wedge. Mr. Nicholson's *Introduction to Natural Philosophy* is the only one of the few works, to which I have access, but what appears to me to make the advantage gained by this instrument twice as great as it ought to be considered.

It is probable that this error (if it be such) found its way into Dr. Olinthus Gregory's otherwise very excellent Treatise, from the theory having but little engaged his attention, being, as he observes, of no very great practical utility. The proposition con-

tained in Article 163 of his Mechanics asserts, that "when a resisting body is sustained against the face of a wedge, by a force acting at right angles to its direction, in the case of equilibrium, the power is to the resistance, as the sine of the semi-angle of the wedge, to the sine of the angle which the direction of the resistance makes with the face of the wedge."

Dr. Gregory proves, that, with respect to the rectangular wedge *a b c*, when it slides freely along the plane *n l*, and the resisting body is sustained against its face *b c*, the power is to the resistance as the sine of the angle *a c b* to the sine of the angle which the direction of the resistance makes with *b c*, and thence infers, that "if the wedge be isosceles, or composed of two rectangular wedges, the force, which in the former case was counteracted by the plane, will now be counteracted by the other half of the wedge; and the power, resistance, and sustaining force, will remain in the same ratio as before."



The latter part of this inference appears to me to be absurd. It necessarily supposes the resistance to be equal to the sum of *two forces*, acting in a similar manner against the opposite sides of the wedge; which is contrary to the terms of the proposition. And moreover, when two bodies are to be separated, by forcing a wedge betwixt them, the resistance of the body which gives way is alone the force requisite to be overcome; the other body merely serves as a fulcrum to the wedge. If the wedge be isosceles, or composed of two rectangular wedges, as *b c h*, and be supposed to slide freely along the plane *m r*, the force, which in the former case was counteracted by the plane *n l*, will now be counteracted by the other half, *a c h*, of the wedge; and as action and re-action are always equal, the power requisite

to keep either half in equilibrio will be the same, and consequently the whole power must now be *twice* as great in proportion to the resistance as before. Or the power will be to the resistance as *twice* the sine of the semi-angle of the wedge to the sine of the angle which the direction of the resistance makes with the face of the wedge, and, consequently, when the direction of the resistance is perpendicular to $a\,c$, as $b\,h$ to $a\,c$.

Or thus :—let the resisting body be urged against the wedge, in the direction $k\,e$; and let it be kept from sliding along the face of the wedge by a force acting in the direction $d\,e$, perpendicular to $k\,e$. There are now three forces acting on the body, namely, the resisting force $k\,e$, the sustaining force in the direction $d\,e$, and the re-action of the wedge in the direction $e\,o$, perpendicular to the surface $b\,c$. Draw $k\,o$ perpendicular to $k\,e$; and, since the three forces are in equilibrio, they will be to each other as the three sides of the triangle $k\,o\,e$. Now, as the wedge is kept in equilibrio by the fore $o\,e$, the power $n\,a$, acting perpendicularly to $b\,h$, and the re-action of the plane $m\,r$, these three forces are to each other as the sides $c\,b$, $b\,h$, and $h\,c$ of the triangle $c\,b\,h$, and therefore $n\,a : o\,e :: \sin.\ b\,c\,h : \sin.\ b\,h\,c$; and because $o\,e : k\,e : \text{rad.} : \sin.\ k\,o\,e$, or $\sin.\ k\,e\,b$, therefore, by compounding, $n\,a : k\,e :: \text{rad.} \times \sin.\ b\,c\,h : \sin.\ b\,h\,c \times \sin.\ k\,e\,b$; or since $\text{rad.} \times \sin.\ b\,c\,h = 2 \sin.\ b\,c\,a \times \sin.\ b\,h\,c$, $n\,a : k\,e :: 2 \sin\ b\,c\,a : \sin.\ k\,e\,b$; as before.

Mr. Nicholson does not take the proposition generally; but after showing that, when the resistance is perpendicular to $a\,c$, the power required to keep one half, $a\,b\,c$, of the wedge in equilibrio, is to the resistance as $a\,b$ to $a\,c$, he (rightly I think) infers, that "as the pressure on the other half of the wedge acts with equal effect, a double force will be required to preserve the equilibrium; that is, a force as $b\,h$ to $h\,c$ [the same as above]. Or, in general terms; in any wedge, as the line $b\,h$, joining the two equal sides $c\,b$ and $c\,h$, is to the distance between the vertex c, and the middle point a of $b\,h$, so is the force [or power] impressed to the resistance." I am, Sir,

Your obedient humble servant,

Evesham, May 11*th*, 1825. I. T.

Art. IX. *On the Transportation of Fish from Salt to Fresh Water.*

[To the Editor of the *Quarterly Journal.*]

Dear Sir,

You expressed a desire to know the progress which has been made in the transplantation of fish from salt to fresh water, since the period at which I communicated the paper on that subject to your Journal. Mr. Arnold, who has carried on these experiments, at my wish, with great zeal, has succeeded in adding many more to the list; and, both in respect to the physical fact, and to the question of economy, the success has been far greater than any one was willing to believe.

The list of the additional fish will be seen by comparing that which is appended to this letter with the former one; and as the subject has excited considerable attention, you will perhaps not object to a statement which may attract even more, by presenting, in the form of a prospectus, the essential facts and arguments. It is only by placing them in this form that they are very likely to produce the effect which appears desirable.

I may now, however, subjoin some remarks which could not well find their place in such a statement, and which have been the result of more experience and attention.

It is certain that the flavour of every fish which has yet been tried has been improved; and I can vouch for the superiority of the basse, the mullet, the loach, the atherine, and the sole, from the pond, to those from the sea. This might be expected, for it is what happens notedly with respect to oysters.

The sole becomes twice as thick as a fish of the same size from the sea, and its skin also becomes extremely dark, or nearly black.

The plaise also increases materially in thickness, and loses its spots. In some cases, it appeared three times as thick as in the sea. The basse also turns much thicker, and improves in delicacy.

The mullet almost ceases to grow in length, but enlarges in breadth, and presents a much deeper layer of fat.

Crabs and prawns have found their own way into the pond; as

have loaches, and some other small fish ; and while, formerly, there were none of the former two, the water is now absolutely swarming with them. Thus also, apparently, the eels have multiplied ; as it is now easy to take a cartload at once, where formerly a dozen or two was a large capture. I have thus also more distinctly ascertained, and to the satisfaction of Cuvier, who had been unwilling to admit it, that there are two species of fresh-water eel, distinguished by the comparative acuteness and breadth of the nose.

I have lastly to add an observation inadvertently omitted in the former communication, which may be used as an *à priori* argument for the possibility of this transplantation. It is, that oxygen is much more easily disengaged from fresh than from salt water. Consequently, the act of respiration ought to be easier in the former than in the latter ; and therefore it is not to be presumed, as it has been, that sea-fish cannot respire fresh water.

As I have given the shad without its Linnæan name, I think it right to add, that our shad is yet unnamed ; because the Clupea Alosa is the Alose of the French, common in the Seine and on the coast of Normandy ; a fish as good as our own shad is detestable, and a decidedly different species of this troublesome and ill understood genus. If I have given the vulgar term rock-fish, it is because I wish to reject the term wrasse, for the present, as it stands for a species ; whereas the whole of this genus (labrus) is still in extreme confusion, and in one, which I hope to aid in rectifying, with the assistance of Cuvier's materials and our own species.

I am yours, &c., J. Mac Culloch.

Prospectus of a Plan for Preserving and Rearing Fish for the London Market.

From various observations and experiments, of which evidence is subjoined, it has been found that sea-fish will live and thrive, and also breed, in ponds or enclosures ; and, with regard to many, it also appears that it is indifferent whether the water is salt, or fresh, or brackish, or alternately fresh and salt.

It is also found that they may be fed in such enclosures, if ne-

cessary, as our domestic animals are; but that if sufficient numbers and kinds are placed together, they feed each other without requiring further care.

It is further observed, that every, or almost every species, improves in flavour and quality, as oysters are known to do under transplantation.

It is well known that, of all the fish brought to market, a very small proportion is in good condition, the rest being apparently ill fed; and hence the number of bad fish so well known to fishmongers.

It is much better known that, from bad weather, or other causes, the supply of the market is very irregular. Thus the public suffers when the supply is short, and the merchant when there is a glut. It is not uncommon for a glut to come in London when the town is empty; and, on the contrary, for it to want fish when full.

The proposed plan, if executed, would bring the fish within our own power, to be taken alive when wanted, and, from being better fed, in greater perfection and more uniformly good. It would be like taking stalled oxen, instead of wild Scotch cattle. It would also enable the merchants to regulate the supply by the demand, and thus to satisfy better the public and themselves. The trade would be steady instead of precarious; as the prices to the public would also be. It would be conducted, directly, between the public and the grower, or with the intervention of one retailer only, and thus a heavy cause of complaint be removed. Lastly, the public would be always sure of fish, and it would be sure of them at moderate prices.

Such are the proposed advantages. As far as the species that breed in such confinement, the fish would reproduce themselves, or a pond of this nature would resemble a common fish-pond. For those that will not, if there are any such, the ponds would be mere repositories for keeping alive, till there was a demand, the fish brought into them from the sea. They would also be feeding places, allowing bad fish to improve. Thus far, the fisheries would go on as before, and the fishing-trade would consequently not be injured. It would be increased, on the contrary;

because, with a better-regulated market and more moderate prices, the consumption would be augmented. There can be no objection therefore on the score of injury to the fisheries. The plan is, virtually, one to preserve fish alive after being taken, instead of suffering them to waste, to render them better in quality, and to supply them more regularly.

The plan, therefore, is, to enclose, in any convenient part of the Thames (since the quality of the water is proved to be indifferent,) a space sufficient for the purpose. A dock, or an excavation in the nature of one, would be unnecessary, as the water itself, in many places not navigated, might be enclosed by a palisade. In this, the fish would be received from the fishermen, by means of well-boats, alive. Those which chanced to die would become the food of others. Many would breed, as they have been found to do, and thus also would produce food. But they might also be fed by means of butchers' offal, or others matters easily procured in a great city, as was the practice of the ancient Romans.

From the enclosure, the fish would be taken by nets, the kinds in demand, and the quantity, selected, and the bad fish also returned for improvement. A steam-boat would supply them to London daily, and to any market which might be established; and they might even be brought up alive, so that the unsaleable ones would not be lost.

The only capital required to be sunk, or advanced, would be in purchasing and enclosing a tract of water, and in the general establishment; possibly, in stocking the pond. It could not be very large; but no estimate is now pretended to be given, nor any place pointed out. That needs not, however, be very near to London, as a steam-boat would approximate any distances. After this, the fish would be purchased from the fishermen by contract; and the establishment, beginning to sell, would then pay its way.

The details of evidence in support of the practicability of this scheme are the following:—

There are three or four sea-ponds in Scotland where fish are thus kept: one in Orkney, belonging to Mrs. Stewart; one on the

Firth of Forth, belonging to Sir Robert Preston; and one in Galloway, belonging to Mr. Macdouall.

On the Greek coast of the Adriatic, at Missolonghi and elsewhere, the same has been practised from immemorial time. It is the current practice also of Bermuda, where the inhabitants subsist chiefly on fish.

These are sea-ponds, as the water is salt. But in Sicily, from the most ancient times also, the natives transport lobsters and crabs to a fresh-water and muddy lake, for the purpose of improving them, as they also do mullet.

With respect to fresh waters, we have evidence of the power of keeping and improving sea-fish in them, from the practice of the ancient Romans. From the testimony of Columella, and the other writers, " de Re Rustica," it was the practice of the Roman farmers, in the earliest days of the Republic, to go down to the sea and bring up the spawn of sea-fish to the fresh-water lakes of Rome, where they multiplied and improved. It was a branch of farming. It became the amusement and luxury of the rich and great in the times of Imperial Rome; enormous establishments of this nature were formed, and the fish were often fed at an expense which, as well as the value of the ponds, proves the great extent of these repositories.

Lastly, this plan has been recently put to the test under the direction of the writer of this note, in Guernsey, by Mr. Arnold. In a pond of about four acres only, many sea-fish, which will be found in the following list, are now thriving, and all those which have had sufficient time have propagated: all have improved in quality, and many very remarkably. This pond was at first worthless, containing only a few eels; at present it produces a large rent, and can supply the market when the weather prevents the boats from going out. It is remarkable also, that, since the introduction of the sea-fish, the eels have multiplied a thousand fold, so as themselves to produce a considerable revenue. This proves that fish may be fed, merely, by bringing different kinds together, as is the case in nature. It may be added, that the evidence from this pond is peculiarly satisfactory, as far as re-

lates to the indifference which sea-fish possess as to the quality of the water. Being embanked from the sea, and receiving an insufficient supply of fresh water in summer, it varies, so that while it is perfectly fresh in winter, it is nearly salt in very dry weather, and brackish in various degrees at intermediate periods. Here also, it is remarkable, that while the larger fishes have been placed there, many of the smaller ones, which formerly showed no such desire, have introduced themselves through crevices in the sea-wall, and that it is, in particular, crowded with crabs and prawns.

It is now necessary to subjoin a list of the fishes which, belonging naturally to the sea, have been found to live in fresh waters. Some of these have been forcibly introduced, others seek it for themselves. If the list is still limited, it is because the rest have not been tried; for no fish on which the experiment has been properly tried, has failed. When they have failed, it is because they were previously injured, or nearly killed, in the taking or the transportation. The *cross* indicates those which have been forcibly naturalized in Mr. Arnold's or some other pond.

Conger	Cod
Torsk	+ Basse
Sprat	Loach
Shad	Red Loach
Alose (of the French) clupea alosa	+ Smelt
	+ Atherine
Greater lamprey	+ Rock fish
Lesser lamprey	+ Cuckoo fish
Stickleback	+ Old Wife
Cottus quadricornis	+ Sole
Mullet	+ Turbot
+ Plaice	Sand eel
Flounder	Rockling
Red flounder, pleuronectes roseus	Whiting pout
	Mackerel
White whale	Herring

+ Horse mackerel	Crabs
+ Pollack	+ Oysters
Prawns	+ Muscles.
Shrimps	

There appears no reason why turtle should not also be cultivated, whether they would breed or not. The peacock, pintado, pheasant, and common fowl, are the natives of hot climates, and have long been naturalized to cold ones; and there is far less difference between the temperatures of the water in different climates than between those of the air. An excellent turtle has been taken in the Tamar at Saltash, after an unknown length of residence.

Art. X. *On the Impurity of the Pulverized Emetic Tartar of the Shops.*

[In a Letter to the Editor of the *Quarterly Journal.*]

Sir,

I am induced to request your insertion in the *Quarterly Journal* of the following facts, with regard to emetic tartar. Having repeatedly noticed a portion of insoluble matter in making the vinum antimonii tartarizati, I purchased some tartar emetic in crystals, and much to my astonishment was charged nearly double what I had previously paid for it in powder. I procured samples, from several respectable druggists, and found in all cases the same inconsistency in price. Upon careful examination, however, of the powder, this was explained, for I found in all the samples, after the triple tartrate had been carefully washed out by cold water, at least ten per cent. and in two or three much more, of a powder comparatively insoluble, and which proved to be principally supertartrate of potash and tartrate of lime. I strongly suspect the manufacturers are in the habit, after boiling the tartar with the oxide of antimony and filtering, of evaporating immediately to dryness that portion which is to be sold in powder; this will explain its impurity, and also its cheapness, when compared with that which has been carefully crystallized. As uniformity

in so active a medicine is of the utmost importance, I would earnestly recommend my brethren of the profession to purchase this article always in crystals. By the insertion of these observations you will much oblige,

Sir, your obedient servant,

AN OLD PRACTITIONER.

ART. XI.—*Some Account of the late M. Guinand, Optician of Brenets, in the Canton of Neufchatel, in Switzerland, read at the Society of Physics and Natural History of Geneva, on the 19th of February,* 1823 *.

THE Society of Physics and Natural History of Geneva, and the Class of Industry in the Society of Arts of that city, having testified in the most flattering manner their approbation of the specimens of *flint glass*, made by M. Guinand, and submitted to their inspection by M. Houriet; and having expressed a desire to possess some details on the origin of his establishment, they are given with much pleasure in the following notice, which is with greater confidence presented, as M. Guinand himself communicated the principal part of the facts which it contains.

* When the present memoir was addressed to the Society, the interesting artist to whom it relates, was still living, but the intelligence of his decease (at the close of 1823,) was communicated at the same time with the history of his labours. The following pages will show how greatly such a loss is to be deplored. After half a century of research, M. Guinand was the only man in Europe who had succeeded in producing large specimens of that *flint glass* which is so indispensable for the construction of achromatic lenses, and at the same time so difficult to obtain, free from striæ, in pieces of any considerable magnitude. Arrangements had been made by the French government for purchasing his secret, when the artist, verging on his eightieth year, died after a short illness. His son remains in possession of his process, and it is said that he will continue to supply opticians with flint-glass necessary for object-lenses of large apertures, the only ones which collect sufficient light, and which produce a sufficiently exact convergence of the rays to allow the application of eye-pieces of a very short focus, or, in other words, to give them a considerable degree of amplifying power.

We may apply to this optician (*si licet magnis componere parva*) a similar remark to that made by the authors of the *Bibliothèque Britannique*, in reference to the celebrated astronomer Herschel, that the facts detailed in this memoir seem to confirm the opinion, that each individual is endowed with a natural disposition towards some determinate pursuit; for it does not appear that the circumstances in which M. Guinand was placed, would have induced him to enter upon the career which he pursued with so much success, if he had not been urged towards it by a peculiar impulse.

Nearly seventy years have elapsed since this interesting man, now on the verge of fourscore, and residing in a remote village among the mountains of Neufchatel, in Switzerland, was employed in assisting his father as a joiner; and his present manner of reading and writing shew that he scarcely obtained the first rudiments of education. At the age of thirteen or fourteen he became a cabinet-maker, and occupied himself chiefly in making clock-cases.

At this period he had become acquainted with a buckle-maker who lived in his neighbourhood, and of whom he learned the art of casting and working in various metals, which enabled him, about the age of twenty, after once witnessing the process, to attempt the construction of a watch-case; having succeeded, he adopted the occupation of a watch-case maker, which was then very lucrative.

Having constructed clock-cases for M. Jaquet Droz, he had an opportunity of seeing, at the house of that celebrated mechanist a very fine English reflecting telescope, which appeared to him extremely curious and interesting. Those instruments were at that time very rare in Switzerland, especially among the mountains. M. Guinand was then in his twentieth or twenty-third year, and it cannot be doubted that this circumstance, in itself unimportant, first turned his mind towards that subject, to which, encouraged by success, he afterwards more particularly devoted himself.

Be that as it may, M. Guinand having expressed a wish to be allowed to take to pieces this telescope, that he might examine it

in detail, M. Jaquet Droz, who had noticed the dexterity of the young man, kindly gave him permission, and with equal good nature relieved him from his apprehension of being unable to put it together again, by taking that task upon himself if it should prove too difficult for him. Thus encouraged, M. Guinand took the instrument to pieces, accurately measured the curves of the reflectors and glasses, and afterwards readily put it together; then availing himself of the few notions of metallurgy which he had gained from his friend the buckle-maker, as well as of the experience he had acquired in casting ornaments for clock-cases, he attempted the construction of a similar telescope, and his second experiment succeeded so well, that on a comparative trial of his own instrument with that which had been its model, in presence of a great number of persons, it was impossible to determine to which of them the preference was due.

M. Jaquet Droz, surprised at this success, asked our artist what treatise on optics he had followed as his guide; but he was still more surprised when the young man told him that he was not acquainted with any; he placed one in his hands, and it was not until this period that M. Guinand studied, or rather deciphered (for, as we have already observed, he reads with difficulty), the principles of that science.

About the same time occurred another fortunate circumstance, in itself as trivial as the former. Having been always weak-sighted, he found, when he began to make watch-cases, that the spectacles, which had hitherto answered his purpose, were no longer of service; and being directed to a person whose glasses were said to have given great satisfaction, he obtained a pair which really suited him no better than the others, but by looking on while they were in progress, he learned the art of forming and polishing the lenses. He therefore undertook to make spectacles, not only for himself but for various other persons, who pronounced them excellent. This new acquirement he found very useful in his favourite pursuit; and he amused himself in manufacturing great numbers of telescopes of an inferior quality, for which he made the tubes himself, generally of pasteboard. He also studied

the small number of works he was able to procure, which treated on subjects connected with optics.

Meanwhile the ingenious and important discovery of achromatic glasses was beginning to spread; and having reached that country, it could not fail to be very interesting to M. Guinand, who listened with avidity to all that he heard on this subject. M. Jacquet Droz, having procured one of these new glasses, permitted M. Guinand, as in the instance of the reflecting telescope, to take it to pieces, and to separate the lenses. It will readily be conceived that the purpose of the latter was to attempt the construction of a similar instrument, but in this he was impeded, by the difficulty of procuring glasses of different refractive power. It was not until some years afterwards that an acquaintance of his, M. Recordon, having proceeded to England, where he obtained a patent for his invention of self-winding watches, which were then in great request, brought him from that country some flint-glass; and though the specimen was much striated, he found means to manufacture from it some tolerably good achromatic glasses. Having obtained supplies of this material on various occasions, and having seen other glasses besides those of M. Jaquet Droz, he easily ascertained that flint-glass, which is not extremely defective, is rarely to be met with. Thus convinced of the impossibility of procuring it of that quality which he ardently wished to obtain for the construction of his telescopes, and having by his various labours become sufficiently skilled in the art of fusion, he melted in his blast furnace the fragments of this flint-glass; no satisfactory result was obtained, but he discovered from some particles of lead, which re-appeared during the process, that this metal was a constitutent in the composition of flint-glass. At the time of this first experiment he had attained his thirty-fifth or thirty-sixth year. The ardent desire to obtain some of this glass then induced him to collect from the different works he was able to procure, such notions of chemistry as might be useful to him in his attempts at vitrification; and during six or seven years (from 1784 to 1790) he employed a part of his evenings in different experiments, melting at each time in his

blast-furnace three or found pounds of glass; he took care, in every experiment, to note down the substances and proportions of his combinations, the time of their fusion, and, as nearly as possible, the degree of heat to which he had subjected them; then, by an attentive examination of the results of his experiments, he endeavoured to discover the causes which had rendered his products defective, in order that he might obviate them on a subsequent trial. While occupied in these researches he derived a strong incentive to perseverance, from the prizes which he understood to have been offered for this desideratum by different academies, and especially by the Royal Society of London, a copy of whose proposals was procured for him. At a later period he also learned, in a more positive manner, from the statements given in the first volume of the *Bibliothèque Britannique*, the almost total impossibility which existed of procuring flint-glass exempt from striæ; all this impressed him with the importance of the discovery at which he was aiming, and stimulated him in the pursuit. These experiments, however, made, as he observed, on too small a scale, all proved fruitless.

At the age of forty and upwards, having relinquished the trade of watch-case maker for that of maker of bells for repeaters, at that time very lucrative, (since he could make as many as twenty-four in a day, for which he was paid five francs each;) he resolved to prosecute his experiments on a more extended scale. Having purchased a piece of ground in a retired place on the banks of the river Doubs, near the Brenets, where his establishment is at present situated, he constructed, with his own hands, a furnace capable of melting at one time two hundred weight of glass, and settled there with his family on a very economical plan, in order to dedicate all his earnings and leisure to new and expensive experiments.

His perseverance, however, had to overcome many untoward accidents, which would probably have deterred most persons from continuing the research. At one time his furnace, which he had not been able to construct with the requisite precautions, threatened to burst while heating, and he was obliged to re-build it

with materials procured from abroad; at another time it was not until after having employed several days, and consumed much wood in heating it, that he noticed an essential defect in its construction, which obliged him to suspend the melting; sometimes his crucibles, which he had procured at great expense, or manufactured himself, cracked without his being able to discover the cause, and the vitreous matter escaped among the ashes, and was lost. After each of these trials he was obliged to employ a longer or a shorter interval in earning the means of subsistence, and of purchasing wood, and the necessary materials for his furnace, his crucibles, and his glass. These fruitless attempts discouraged him on some occasions, but on others excited him so as to deprive him of rest, and he meditated day and night on the probable causes of the accidents, and on the means of obviating them. At length, however, he obtained a block (*culot*) of glass, of about two hundred weight; having sawed this block vertically, he polished one of the sections, in order to examine what had taken place during fusion, and the following were the appearances:—On the upper surface of the vitreous matter there were many little semi-globules, which had the appearance of drops of water, terminating by a thread or little tube of greater or less depth, at the extremity of which there was a small spherical bulb. The cause of this appearance was, that these drops and tubes consisted of a denser kind of glass than the rest of the block. In another part there arose from the bottom of the crucible other cylinders, or tubes, terminating also in a kind of swelling or bulb; these had a hollow appearance, because they were formed of a substance less dense than the rest of the glass; and lastly, here and there were seen specks, or grains, ending with a tail or train of a substance also less dense than the rest of the mass in which they floated; these, on account of their appearance, he denominated *comets*. In evidence of this result we still possess a specimen of the glass which M. Guinand formerly sent to us. The block here spoken of has long ago disappeared.

The following is M. Guinand's explanation of these effects:—Having often seen on the surface of his glass small globules of

lead, he supposes that, from some cause or other, certain particles of the lead which composes his vitreous matter separate from it, and appear on its surface in their metallic state; that becoming again oxydated by contact with the air, or re-calcined after being *resuscitated*, (to use his own expression,) they combine with the vitreous matter on which they rest, and thus form in the place where they occur, that glass of greater density which appears on the surface in the form of drops. The specific gravity of this substance draws it to the bottom of the crucible; but in descending more or less slowly, according to the temperature of the furnace, it leaves in its passage a train which occasions those threads of glass that possess a stronger refraction. Having reached the bottom, this vitreous matter, in some degree saturated with minium, being a powerful solvent, attacks the substance of the crucible, and forms with it a vitreous compound, of an inferior density to the mass, and ascending in consequence of its specific levity, produces those cylinders or tubes formed of a less refractive glass. Lastly, when this solvent, by melting the substance of the crucible, especially that of the bottom, has detached from it a grain of sand or baked clay, this half molten grain rises and floats in the mass in an oblique direction, because, being still attached to a part of the vitreous matter which it has produced, it is not actuated on all its points to ascend with equal rapidity *.

Whatever may be thought of this explanation, the original cause of the non-homogeneity of strongly refractive glass being once ascertained, the question was, how to remedy it; and it was

* This explanation having been communicated to M. Breguet, a Swiss artist, who was subsequently attached to the Board of Longitude at Paris, but who at this period was still residing in his own country, he was of opinion, that to obtain homogeneous flint-glass, it would be sufficient to melt it in a covered crucible of platina; he consequently transmitted to M. Guinand a small plate of that precious metal, but the latter did not coincide in M. Breguet's opinion; he foresaw that the solvents would act sufficiently on the metal to colour the glass, and his conjecture was justified by an experiment made in a very small crucible. However, when M. Breguet went to reside at Paris, he announced to the public, that the cause of the defects in flint-glass being known, as well as the remedy, it would thenceforward be easily obtained in a perfect state.

here in particular that M. Guinand had great obstacles to surmount; so that, said he, the sacrifices and exertions which he had previously made, were trifling compared with those which he afterwards underwent for the purpose of removing these various defects, and of rendering his glass homogeneous.

Here we would gladly relate the numerous experiments by which M. Guinand at length accomplished his grand discovery; but, as it still serves to procure for him some compensation for his labours, we should be unworthy of the confidence he has reposed in us were we to enter into any detail on this subject. We shall therefore only state, that after many expensive trials, M. Guinand having been so fortunate as to obtain glass of which some parts were perfectly homogeneous, and therefore destitute of those striæ or threads from which flint-glass is so rarely found free, he reflected on the different circumstances which, in this experiment, might have contributed to so happy a result, so that in subsequent attempts he obtained blocks of glass possessing larger portions of homogeneous substance, and at length he has almost arrived at a certainty of obtaining in the fusion of from two to four hundred weight of glass, at least one half of that substance perfectly homogeneous, and consequently fit for optical purposes.

Unable to make any further progress, he admits that his processes have not yet attained all the perfection which might perhaps be desired; but, as he has by these means succeeded in making disks, perfectly homogeneous, of twelve, and in one instance even of eighteen, inches in diameter*; and having no

* While occupied in his laboratory, M. Guinand never permitted any persons to be present, except his wife and son, who assisted him. On these occasions they were generally secluded for many days and nights from society; but when M. Guinand had nearly completed his operations, and had arrived at a favourable result, his friends and neighbours were admitted, and partook of some refreshment, while offering their congratulations on the termination of his confinement.

A year or two before his death, M. Guinand tried an experiment on a larger scale than he had previously attempted. After much trouble and exertion he succeeded in obtaining a disk of eighteen inches in diameter, of perfectly ho-

doubt that, in operating on a greater scale, he might easily be able to obtain one of a diameter double or triple the extent of those last mentioned, he justly concludes that his process has at length removed the obstacle which the non-homogeneity of flint-glass opposed to the construction of large achromatic object-glasses*.

When M. Guinand first obtained blocks including portions of good glass, his practice was to separate them, by sawing the blocks into sections that were horizontal, or perpendicular to their axis; then, polishing the sections, he selected the portions adapted to his purpose, and returned the others to the crucible; but, independently of the tediousness of the labour, and the waste occasioned by sawing, this process was attended with the great disadvantage of not cutting the finest parts of his glass in the manner best calculated to obtain disks as large as possible; for frequently the most homogeneous parts were thus divided; but a fortunate accident, of which he availed himself with his usual adroitness, conducted him to a process more simple and better suited to the attainment of his object.

One day, while his men were carrying a block of this glass on

mogeneous glass. This disk had been put into the oven for the last time, to be gradually cooled; and the operation being now considered as completed, the friends were, as usual, admitted; in the midst of the congratulations offered, on this unprecedented success, after an unusually long seclusion, the fire, by some accident or neglect, caught the roof of the building. On this alarming occasion all present exerted themselves, and after some trouble the flames were extinguished; but not before some water had found its way into the oven, and destroyed its precious contents. The discouragement caused by this misfortune, and some other circumstances, ever after prevented M. Guinand from attempting any new experiment on a similar scale.

* "At the Exposition of the French Industry, several large telescopes were exhibited by MM. Lerebours and Cauchoix, constructed with flint-glass, made by M. Guinand, of Neufchatel. One made by Lerebours had its object-glass nine inches in diameter, French measure, and its focal length ten and a half feet; and another by Çauchoix, had the diameter of the object-glass eleven inches, and the focal length eighteen feet."—*See the Edinburgh Philosophical Journal*, No. XX. for April, 1824.—*See* also *Sc. et Arts*. Nouv. Série, Vol. XXV. No. 2. Feb. 1824.

a hand-barrow to a small water saw-mill, which he had established at the fall of the Doubs, at the distance of half a league from his abode, the mass slipped from its bearers, and rolling to the bottom of a steep and rocky declivity, was broken into several pieces; M. Guinand was at first grieved at this mishap, but, having selected those fragments which appeared to be perfectly homogeneous, he succeeded in softening them in circular moulds in such a manner that, on cooling, he obtained disks that were afterwards fit for working. Thenceforward he adhered to this method; and he also contrived a mode of cleaving his glass while cooling, so that the fractures should follow the most faulty parts. At his house are frequently to be seen pieces of unwrought glass, weighing from forty to fifty pounds, perfectly transparent and homogeneous. When flaws occur in these, he removes them by cleaving the pieces with wedges; he then melts them again in moulds which give them the form of disks, generally taking care to allow a little of the glass to project beyond one of the points of the edge, so that the opticians who work them may be enabled to use that portion of glass in making a prism, which shall give them the measure of the refractive power, and thus obviate the necessity of cutting the lens. For the refraction of M. Guinand's glass varies almost at every casting, while, on the other hand, that of each casting is of such homogeneity, that the refractive force of two pieces taken indifferently, one from the top and the other from the bottom of the crucible, is absolutely the same. This mode of fabricating glass from pieces rough and shapeless in the first instance, and afterwards re-molten into disks, renders the process of M. Guinand absolutely different from that of other glass-founders, who proceed either by casting or blowing.

When the disks obtained by this process are still defective, as it sometimes happens, especially with those of great dimensions, M. Guinand obviates the defects by means of the wheel (*roulette*); then by softening them again the vitreous matter expands, and fills up the hollows that have been made; if after polishing he finds them still defective, he repeats the process until the disks are as perfect as he could wish. By these means he has often

succeeded in soldering pieces of glass which have left no trace of their separation: at first these pieces were only cemented; there was frequently even air or sand between the united surfaces; in these cases, he cut along the line of junction a small semi-cylindrical groove, in order that the vitreous matter, while melting, might fill it, not by flowing from its edges to the bottom, but by raising the bottom itself; and, by repeating this operation a sufficient number of times, he declares, that he has succeeded in totally effacing all traces of junction.

In 1798 or 1799, M. Guinard, having visited Paris, presented to several men of science, and among others, to the late M. de Lalande, several disks of from four to six inches, of the glass which he obtained in sawing his blocks, (not having at this period thought of the expedient of re-melting them;) that celebrated astronomer valued them highly, and having inquired of M. Guinand what he intended to do with them, the latter expressed his wish that they might be made into object-lenses, in order to ascertain if the glass which he procured by means of his process was of the kind that had been so long desired; but M. de Lalande having understood that he was conversant with optics, advised him to work them up himself, so as to demonstrate the goodness of his glass. M. Guinand followed this advice, and while continuing his manufacture of bells for repeaters, he pursued for several years the making of glass and the working of lenses; he constructed achromatic telescopes, some of which had object glasses of four or five inches, perfectly free from striæ, and having purchased a small water-mill at Brenets (where he still resides) he adapted it to the polishing of his glass. Notwithstanding the advice of M. de Lalande, he made no effort to obtain the prizes offered for his discovery, because, while he was still not sufficiently convinced that his glass possessed all the qualities required, (which cannot be positively ascertained until it is wrought) the proposals of one of those societies, of which he possessed a copy, requiring that the whole detail of the processes by which it is obtained should be revealed to different committees, before any decision is made, he was apprehensive that some dif-

ficulty might deprive him not only of the prize, but of the profit which he might fairly derive from his sacrifices, by the exercise of an industry in which he had no competitor.

Though his success was not publicly known, yet he had acquired, by means of his different labours, and especially by his manufacture of achromatic telescopes, a sufficient degree of reputation to be visited by men of science who travelled into his country; having thus became acquainted with Captain Grouner, of Berne, an intendant of the mines, the latter had occasion in Bavaria to speak of the labours of M. Guinand, and a short time afterwards, in 1804, he asked him, on the part of M. Frauenhofer, the chief of the celebrated establishment of Benedictbeurn, for some specimens of his glass. The letter of M. Grouner at once testifies the high esteem he had conceived for M. Guinand, and his earnest wish that his discovery might be rendered useful. M. Frauenhofer after examining these specimens, and requesting several disks of the glass, was so well satisfied with them as to repair in person to Brenets, a distance of about 260 miles, where he engaged M. Guinand to take a journey into Bavaria: having arrived in 1805, he determined to settle there; and during a residence of nine years he was almost solely occupied in the manufacture of glass. It is from this period that M. Frauenhofer's achromatic telescopes have acquired so well-merited a reputation.

Returned to his native country, he again established himself at Brenets, where he is often visited by foreigners, attracted by the beauty of the situation, and the view of the waterfall of the Doubs. After having discontinued for several years subsequent to his return all his labours relative to optics, his taste for the pursuit revived, and from that time his entire occupation was alternately the manufacture of glass and the construction of telescopes, in proportion as either object appeared likely to prove most profitable.*

* Being at a very advanced age, M. Guinand wished to return to the land of his birth, and a pension was granted to him by the establishment of Benedictbeurn, on condition that he should no longer employ himself in making glass, nor disclose his process to any person whatever. After his return he resided at

Among the opticians who have used his glass, and whose judgment cannot be questioned, may be particularly mentioned M. Lerebours, an artist attached to the Board of Longitude at Paris, who, during a visit to Brenets in 1820, having obtained all the glass which M. Guinand then had in his possession, though at that time it was not so perfect as he hoped to make it by successive re-softenings, was so well satisfied with it that he not only requested a fresh supply, but was induced to make overtures for obtaining a communication of the process. We may also adduce M. Cauchoix*, who, in a notice relative to the telescopes placed in the last exhibition at the Louvre, has spoken of the flint-glass, of which they are constructed, in a manner which at once shews the goodness of M. Guinand's glass, and the good faith of the French artist. When the *Bibliothèque Universelle* announced the formation of the Astronomical Society of London, M. Guinand was requested (in 1821) to present to them a sample of his glass, upon which they were pleased to make a report as favourable as the small size of the specimen could warrant; they also very graciously offered another, on disks of a larger dimension. M. Guinand accepted the offer, and they have in progress a disk of seven inches, similar to those which have been

the Brenets for two years in a state of inaction ill suited to the activity of his genuis. Confident that by new experiments he could raise his discovery to a higher degree of improvement, he wrote to Bavaria, requesting that on relinquishing his pension he might be released from the engagements which he contracted. His request having been granted, he ardently resumed his labours, and it was during this last period of his life (a space of nearly seven years), that this ingenious and persevering artist succeeded in making those disks of eleven and twelve inches English measure, which are wholly free from defects, and which excite the astonishment and admiration of all who are acquainted with the difficulty of procuring disks of flint-glass of even five inches in diameter, fit for optical purposes.

* Some time after M. Lerebours's visit to Switzerland, M. Cauchoix having been accidentally informed that M. Guinand was the person who made this excellent flint-glass, without a moment's delay posted to Brenets, a distance of 800 miles, and not only purchased all the glass which was actually manufactured, but ordered those disks of extraordinary dimensions which we have just noticed.

submitted to the inspection of the learned societies to which his notice is addressed; and that disk, now in the hands of the first artists in London, will be the subject of one of their earliest official reports*.

Among the telescopes made by M. Guinand after his return to his native country, there are several of remarkable magnitude and effect; in general, the greater part appear to advantage on comparison with English telescopes; a merit which is owing in an especial manner to the quality of the glass. But the most singular circumstance attending them is, that they have been constructed by an old man upwards of seventy, who himself manu-

* Extract from the Report of the Council of the Astronomical Society of London, held the 14th of February, 1824.

"This Council have instituted some experiments on the properties of glass, for the formation of the object-lenses of refracting telescopes. With this view a thirty-inch telescope was constructed of some foreign glass (sent by M. Guinand) for a trial of it, by Mr. Tulley. The result was satisfactory, and the telescope has since been sold to one of the members. Three other pieces of glass, presented to the Society by M. Guinand of Neufchâtel, (one of which is of a very large dimension, and promising aspect), are now undergoing trial in the hands of a committee, to which the examination of their merits has been referred."

With that disk Mr. Tulley has constructed a twelve feet focal length telescope, with which fixed stars are seen with extraordinary clearness and brilliancy. In his report to the Astronomical Society on the subject, dated Jan. 12, 1825, Mr. Tulley says, that owing to some peculiar circumstances he has not yet quite succeeded in working the glass to his mind; and adds, "but I have no doubt I shall be able to make it into a very perfect instrument; the glass being entirely homogeneous and free from fault; the material of the glass appears to me to be different from our flint-glass, as it grinds and polishes much easier; but whether it be harder or softer, or of a more brittle nature, is of little or no importance. The ratio of the refractive and dispersive power compared with the specific gravity, seems to be lower than that of the refractive and dispersive power in the British flint-glass, compared with the same specific gravity.

"I have another piece of flint-glass 3¾ inches, of the same manufacture, that seems likewise to be quite free from fault, and is as clear all over as any fluid."

It is understood that the committee will make their report upon this lens, at the next meeting of the Astronomical Society.

factures the flint and crown-glass which he uses in their construction, after having made with his own hands his vitrifying furnace and his crucibles, who, without any mathematical knowledge, devises a graphic method of ascertaining the proportion of the curves that must be given to the lenses, afterwards works and polishes them by means peculiar to himself, and lastly, constructs all the parts of the different mountings either with joints, or on stands, melts and turns the plates, solders the tubes, prepares the wood, and compounds the varnish.

[Here follows a long and detailed explanation of the original process pursued by M. Guinand for giving the required curve to his lenses; but as the mode adopted in this country appears to be superior, that detail is omitted.]

In beholding M. Guinand in his seventy-sixth year, assailed with the infirmities incident to his multifarious labours and his advanced age, infirmities which have latterly been so severe as to disable him from all exertion, it is to be regretted that such a man cannot enter upon a new career, it is to be lamented that, after having sacrificed to his art so much more than could have been expected from a man in his circumstances, he should derive from them so little advantage; and lastly, it is painful to think, that this man, in attaching so little importance to the honour of his discovery, should not have made it more extensively known, and connected it more closely with his name; since it is a discovery which, by the perfection it imparts to telescopes, opens the way to very important acquisitions in the vast field which the heavens still offer to optical instruments in a state of perfection.

E. R.

Dated *Neufchâtel, Jan.* 1824.

ART XII. *Theorems in the Doctrine of* ANNUITIES. *By a* Correspondent.

WHATEVER may be the difficulties in estimating the rate of interest for small portions of the year, which have perplexed many of the most popular authors on annuities payable at short intervals of

time, they may all be superseded by a very simple mode of considering the subject, derived from the progressive fluctuation to which every common annuity must necessarily be liable.

It is perfectly well known, that stocks of every kind, besides their accidental variations, are subject to gradual elevations in their true values, during the intervals of the payment of the dividends, and to a sudden depression, the moment that each dividend has been paid, equal in magnitude to the amount of the dividend: and it is equally obvious, that a life annuity becomes continually more valuable, as the period of the payment approaches, and loses at once the value of one payment the moment that payment has been managed.

An annuity, for example, of which a payment is due on a given day, is more valuable than an annuity purchased on that day, and payable a year after, by the amount of a year's payment: and "*The value of an annuity beginning to be payable at any intermediate time between the day of purchase and its first anniversary, will be greater than the simple tabular value of the annuity by a sum proportional to the anticipation of the payments,*" the increase of the value being very nearly uniform, when we suppose the anticipation to be gradually increased: this increase of the value depending obviously on the greater probability as well as on the greater proximity of each payment, and proceeding from day to day by very nearly equal increments. Thus if we wished to purchase an annuity of 100*l.* a year, and its value were 1000*l.*, upon the ordinary supposition of the payments commencing after the end of a year; supposing that we desired to have the first payment made at the end of nine months, and the subsequent payments at intervals of a year as usual: we should have to add 25*l.* to the purchase money, making it 1025*l.* at whatever rate of interest the value might have been computed. If we began at six months, 50*l.*, and if at three months, 75*l.* must be added to the purchase: it being obvious that an additional 100*l.* would be equivalent to an anticipation of twelve months, or to an immediate payment of a year's annuity.

From this simple and incontestible principle, it is very easy to

deduce the values of annuities, payable at intervals shorter than a year. An annuity of 1, payable half yearly, is equal to two annuities of $\frac{1}{2}$, the one beginning as usual at the end of the year, the other anticipated by half a year; and the value of this portion is greater than the other by half a year's payment, that is, by $\frac{1}{4}$: so that "*We may always find the value of a life annuity payable half yearly by adding a quarter of a year to the tabular value of the same annuity.*"

In the same manner, an annuity payable quarterly may be divided into four parts, the value of the three, which become payable before the year's end, being greater than the fourth by $\frac{1}{4} \times \frac{3}{4}$, $\frac{1}{4} \times \frac{1}{2}$ and $\frac{1}{4} \times \frac{1}{4}$, respectively; and the sum of the whole being greater than the simple value of the annuity by $\frac{3}{16} + \frac{2}{16} + \frac{1}{16} = \frac{6}{16} = \frac{3}{8}$, and

"*For quarterly payments, we must add* $\frac{3}{8}$ *of a year's value to the computation made on the supposition of annual payments.*"

It is easy to show, by continuing the operation, that the limit of this anticipation of the payment, by the continual bisection of the interval, would at last afford us the addition of half a yearly payment for the value of a daily or hourly payment of a proportional part of the given annuity.

It may also be observed, that when we reckon at three per cent. per annum, an annuity payable half yearly is nearly the same that would be granted on a life a year older, if payable annually: a year and a half, if we suppose the payment to be quarterly; and two years, if daily or hourly.

A. B. C. D.

7, Waterloo Place, 10 *June*, 1825.

Art. XIII.—*Proceedings of the Royal Society.*

April 28.—The reading of Dr. Granville's "Description of Egyptian Mummies, with some Remarks on the Art of Embalming," was resumed and concluded.

The mummy described in this paper was purchased at Gournou. It was in a single case, of the usual form, and covered with cere-cloth and bandages very neatly and dexterously applied, and among which both cotton and linen were recognised—these, to the amount of 28lbs. avoirdupois in weight, having been removed, the body was discovered to be that of a female. The abdominal integuments were remarkably wrinkled, and the whole surface of a dark brown colour and dry, but in many places soft to the touch, and with the exception of a few parts, entirely deprived of cuticle. The height of the mummy from the vertex of the head to the inferior surface of the calcaneum was 5 feet $\frac{7}{10}$ inch, and the principal dimensions of several parts correspond with those which are usually considered as giving rise to the utmost perfection of female form in the European race; neither was any *trait* of Ethiopian character discernible in the form of the cranium; all which, observed Dr. Granville, supports Cuvier's opinion respecting the Caucasian origin of the Egyptians.

The author then proceeded to a brief summary of the present state of our information respecting Egyptian mummies, attributing its scantiness and imperfection to the rarity of perfect specimens, nearly all the mummies hitherto described presenting little else than imperfect skeletons enveloped in bandages, sometimes covered by the dry skin.

In proceeding to examine the present specimen, the integuments and muscles of the abdomen were first removed, and the contents of that cavity carefully inspected; they consisted of a portion of the stomach adhering to the diaphragm, the spleen attached to the superrenal capsule of the left kidney, and the left kidney itself with the ureter descending into the bladder, which, with the uterus and its appendages were observed *in situ*, the latter exhibiting marks of disease.—Fragments only of the intestinal

tube were discoverable, and there were a few lumps of resin, of a compound of clay and bitumen, and a few pieces of myrrh. The right kidney, the liver, and minor glands were missing; but the gall-bladder was detected among the loose fragments of membranes and other soft parts, together with remains of its own ducts. The soft parts of the pelvis were then particularly examined, and the perfect condition of the muscles, membranes, and ligaments, particularly noted. The cavity of the thorax was next examined, by detaching the diaphragm to which part of the pericardium adhered, and the heart in a very contracted state was afterwards found suspended by its vessels and attached to the lungs, which adhered to the ribs.

Upon the examination of the cranium, it was evident that the brain had been removed through the nostrils, from the lacerated condition of the inner nasal bones; the eyes appear not to have been disturbed, the tongue was entire, and the teeth white and perfect.

Dr. Granville next proceeded to draw some conclusions as to the age at which this mummied female died, and respecting the disease which destroyed her. The bones of the ilium exhibit that peculiar thinness of their osseous plates which show the individual to have exceeded her fortieth year and to have borne children; and as there are no characters of age or of decrepitude about the skeleton, the author considers her to have been about fifty. The ovarium and broad ligament of the right side were enveloped in a mass of diseased structure, while the fallopian tube of the same side was sound; but the uterus itself was larger than natural, and the remains of a sac was found connected with the left ovarium, all which, connected with the appearance of the abdominal integuments, leave no doubt of ovarial dropsy having been the disease under which the individual suffered.

The author concluded this communication with some observations respecting the method of embalming generally, and the nature of substances employed in the process, from the details of which he drew the conclusions following:—

That the abdominal viscera were more or less perfectly ab-

stracted either through an incision on one side of the abdomen, or, as in the present mummy, through the anus. The thoracic cavity was not disturbed. That the contents of the cranium were removed; sometimes through the nostrils, and in others through one of the orbits. The body was then probably covered with quick-lime to facilitate the removal of the cuticle; the scalp and nails being however left untouched; after which it was immersed in a melted mixture of wax, resin, and bitumen, until thoroughly penetrated; and, ultimately, subjected to a tanning liquor, probably made with the saline water of the neighbouring natron lakes; the bandages were then applied, with the occasional interposition of melted resin, or wax and resin, the lumps of resin, myrrh, &c., having been previously placed in the abdomen.

M. Gay-Lussac took his seat, as a foreign member of the Royal Society.

Henry Harvey, Esq., was admitted a Fellow of the Society.

May 5.—Dr. H. H. Southey was admitted a Fellow of the Society.

May 12.—John Taylor, Esq., was admitted a Fellow of the Royal Society, and a paper was communicated by Peter Barlow, Esq., F.R.S., *On the Magnetism imparted to Iron Bodies by Rotation.*

The author's attention having been recalled to the consideration of the effects of rotation in altering the magnetic influence of iron, in the course of speculations on the cause of the rotation of the earth's magnetic poles, and knowing at the same time that Mr. Christie had found a permanent change in the magnetic state of an iron plate by a mere change of position on its axis, it seemed to him highly probable that this change, due only to a simple inversion would be increased by rapid rotation. On trial, however, it was found that the effect produced was merely temporary. The experiments at first were made with a thirteen inch mortar-shell fixed to the mandril of a powerful turning lathe, worked by a steam engine in the Royal Arsenal at Woolwich.

This being made to revolve at the rate of 640 turns per minute, the needle was deflected out several degrees, and there remained stationary during the motion of the ball, but returned immediately to its original position on ceasing the rotation. On inverting the motion of the shell, an equal and contrary deflection took place.

As the law of the phenomena was not evident with this disposition of the apparatus, and the shell was found too heavy for perfect safety, a Shaprnell shell of eight inch diameter was mounted in a proper apparatus (described in the paper,) and a number of experiments made, the law of which, however, still seemed anomalous, till the idea occurred of neutralising the earth's action on the needle, when the anomalies disappeared, and the general law of the effect was placed in evidence. The needle being made a tangent to the ball, if the motion of the ball was made *towards* the the needle (whatever was the direction of the axis of rotation,) the north end of the latter was attracted, and if the contrary way, repelled. In the two extremities of the axis there was found no effect; while in two opposite points at right angles to the axis, the effect was a maximum, and the direction of the needle was to the centre of the ball.

The author then proceeded to shew how the results, which before appeared anomalous, agree with this general view, and closed his communication with some theoretical views of their general bearing on the subjects of the earth's magnetism, which he thought there were strong reasons for believing to be of the *induced* kind, and although it appeared to him doubtful whether the anomalies observed in the variation of the needle on the earth's surface, can ultimately be referred to this cause, yet he observed that one condition essential to the production of these phenomena, holds good in the case of the earth, *viz.*, the non-coincidence of its polarized axis with that of its diurnal rotation.

A paper, *On the Alteration in the Magnetism of an Iron Plate, occasioned by a Rotation on its Axis, by* J. H. Christie, Esq., was communicated by the President.

The effects observed and described in this paper, although mi-

mute in themselves, appeared, in the author's opinion, to point out a species of magnetic action not hitherto described. It has long been well known that striking, twisting, or filing iron, in different directions, with regard to the magnetic axis, materially influences its polarity, but it does not appear to have been remarked that the simple rotation of iron in different directions has any such influence. This, however, the author has ascertained to be the case, and that the laws which govern this peculiar action are so regular, that there can remain no doubt of a corresponding regularity in their causes.

The attention of the author was first drawn to these phenomena by some apparent anomalies in the magnetic action of an iron plate on the compass, observed in the course of a different investigation. In order to avoid or allow for the disturbing influence of partial magnetism in the iron, it became necessary to attend minutely to the position of certain points in its circumference, which corresponded to the maxima and minima of this magnetism. It was then found that these points were not constant, but shifted their position as the plate was made to revolve in its own plane; or, in other words, that a plate which, in a given position, produced a certain deviation in a compass, no longer produced the same deviation after making an exact revolution in its own plane, although brought to rest, and every part of the apparatus restored precisely to its former place.

It appeared from this, that the revolution of the plate in its own plane had an influence on its power of deviating the needle independent of the partial magnetism of particular points in it, and the justice of this idea was proved by giving it a rotation in an opposite direction, when the effect on its directive power was also reversed.

The change produced by rotation in the directive power of the plate was found to be a maximum when its plane was parallel to the line of dip on the magnetic axis, and at the same time as little inclined to the horizon as this condition would allow; but when the plane of the plate was parallel to the horizon the effect was diminished in the ratio of 5 to 1; and when perpendicular to the

horizon; and coincident with the magnetic meridian, was altogether destroyed.

The author, having satisfied himself of the reality and constancy of this effect, in different plates, and of the necessity of referring it to a peculiar agency of the earth's magnetic power on the molecules of the plate, proceeded to ascertain the laws, and measure the quantities of the *deviation* due to rotation (so he terms it) in various positions; and detailed a great number of experiments, with their numerical results, arranged in the form of tables.

From these he deduced the following general law; *viz.*, that the deviation due to rotation in *a dipping needle* "will always be such, that the sides of the equator of such dipping needle will deviate in a direction contrary to the directions in which the edge of the plate moves, that edge of the plate nearest to either edge of the equator producing the greatest effect."

The results of this law, it may be here observed, are in many cases coincident with those of the following: conceive the dipping needle orthographically projected on the plate. Then will the *deviation due to rotation* of the projected needle take *place in a direction opposite to that of the rotation itself.*

The author then proceeded to a theoretical investigation of the effect of a plate of soft iron, having within it two poles developed in given positions, and acting (in addition to the usual magnetic action of soft iron) on a needle of infinitely small dimensions, in the plane of the plate. He referred the whole ordinary action of the iron to its centre, and supposed that this is *attractive* on both poles of the needle; but the extraordinary action on that of the newly-developed poles he supposed to reside in them, and to be attractive or repulsive according as they act on the poles of the needle of the same or opposite names with themselves. On this hypothesis, assuming symbols for the coordinates of the plate's centre, the distance separating the newly-developed poles in the plate, and the angle which the line joining them makes with the direction of the needle, *&c.*, he deduced (from the known laws of magnetism) formulæ, expressing the horizontal deviations of the needle;—first, on the supposition of

a rotation in one direction; secondly, on that of a rotation in the opposite; and thirdly, in that of no rotation at all. From these, by comparing them with a few of the observations, he deduced numerical values for the constants of the formulæ, and then employed them to compute the deviations due to rotation in all the rest. He regarded the discrepancy between the calculated and observed results, as in few cases, larger than what he considered may be fairly attributed to error of observation; and that the theory above stated is at least a general representation of what passes in fact: admitting, however, that it does not give the exact position of the point where the deviation due to rotation vanishes, and suggesting partial magnetism in the iron plate used as one mode of accounting for the difference. At all events, by an examination of the case on the ordinary supposition of induced magnetism in the iron, he shewed that a greater coincidence between theory and fact would not result from that hypothesis than from the one here employed.

He then proceeded to inquire into the degree of permanence of the polarity thus produced in iron by rotation, from which inquiry it appeared that (at least during 12 hours after the plate was brought to rest) the influence of a single rotation had scarcely suffered any diminution. It appeared also that the effect is so far from depending on the rapidity of the motion, that the plate can scarcely be made to revolve so slowly as that the whole effect shall not be produced.

Lastly, by a slight change in the formulæ, the results of computation it is found can be made to agree with observation to a degree of exactness as near as can be wished. This change consists in the omission of certain terms introduced by the theory, and the author regards it as very possible so to modify the theory as to get rid of them.

The author closed this communication with an appendix comparing the magnetic effects produced by slow and rapid rotation. The result of the comparison was, that the forces exerted on the needle during rapid rotation were always in the same direction as those derived from the slowest rotation, and which continue to

act after the rotation has ceased, but were greater in intensity, and that the former effects were such as might have been looked for from a knowledge of the latter.

May 19.—George Harvey, Esq., John Smirnove, Esq., and the Rev. Dr. Morrison, were admitted Fellows of the Society.

A paper, entitled *Some Account of the Transit Instrument lately put up at Cambridge Observatory, by* Robert Woodhouse, Esq., F.R.S., was communicated by the author.

The author in this paper first described the operations by which the New Transit Instrument, at the Observatory of Cambridge, was approximately placed so as to allow of a meridian mark being erected on the distant steeple of Granchester church. He then entered into a more full consideration of the different methods proposed and employed by astronomers for executing the more delicate adjustments of the Transit in general—he shewed how the errors of collimation, level, azimuth, and the clock, may all be detected, and their values determined by the resolution of certain equations of the first degree, constructed from observations of any three or more stars; but this method, though exact in theory, he reprobated in practice, and prefers making each adjustment separately, and by the ordinary mechanical trials, as shorter, more effectual, and less troublesome. Mr. Woodhouse then described a remarkable phenomenon presented to him by the transit in the course of his observations. He found that the line of collimation of the instrument deviated occasionally to the east or west of the centre of the meridian mark without any apparent reason. At length, however, it was found that this was caused by the approach of the assistant's body to the lateral braces placed for the purpose of steadying the instrument in an invariable position at right angles to its axis. The expansion of the brace nearest to him was found to thrust the axis of the telescope aside, and on the removal of the assistant, the equilibrium of temperature restoring itself, the deviation gradually disappeared. That this was the true cause appeared by wrapping hot cloths round the

alternate braces, by which the same effect was produced in an increased degree. Warned by these observations, Mr. Woodhouse ordered a proper apparatus to be provided to defend the braces from the sun's rays during the meridian passage of that luminary.

A Description of the Fossile Elk of Ireland, by Thomas Weaver, Esq., communicated by the Rev. W. Buckland, F.R.S., was also read.

Mr. Weaver's principal object in this paper is, to prove that the remains of the gigantic elk, which have been found in various parts of Ireland, are not of antideluvian origin, but that the animal dwelt in the countries in which its remains are now found, at a period of time which, in the history of the earth, can be considered only as modern; and that the extinction of the species is attributable rather to the continued persecution it endured from its enemies, accelerated by incidental local causes, than to any general catastrophe that overwhelmed the surface of the globe.

The spot examined by the author, containing these remains, is near the village of Dundrum, in Down. It appears formerly to have been a lake, and is now covered with peat, lying upon a bed of marle; the bones are invariably found between these two substances, and from the examination of the shells contained in the latter, it appears that they are exclusively fresh-water species.

The peat bog of Rathcannon, in the county of Limerick, has also furnished abundance of the same bones, similarly situated. These were examined by the Rev. Mr. Maunsell, before they were displaced. Some of them shewed marks of disease and fracture, and in one case, the rib was singularly perforated, as if by a sharp instrument. Marrow, having the appearance of fresh suet, was found in the cavity of one shank-bone, and they appeared, generally, to contain all the principles found in fresh bones.

These, and some other concurrent circumstances, seem, says the author, to remove all idea of the remains of the Irish elk being of any other than, comparatively, recent origin; and in

seeking for a cause of the nearly constant distribution of these remains in Ireland, in swampy spots, he conjectures that the animal may have often sought the waters and the marshy land, as a place of refuge from its enemies, and thus not unfrequently found a grave where it looked for protection.

June 2.—A paper, entitled *Microscopical Observations on the Materials of the Brain, and the Ova of Animals, and the Analogy that exists between them*, was communicated by Sir E. Home, Bart., V.P.R.S.

The author first detailed the results of some experiments, made with a view to ascertain whether frogs that had been completely frozen, could, under any circumstances, be restored to life; which he found never to be the case, where the brain had been entirely congealed, the substance of which, after such a process, never regains its former appearance, but is resolved into a watery fluid, mixed with some gelatinous matter. In the act of freezing, the human brain was found to suffer a similar decomposition. The molecule of the egg is also resolved, during the process of freezing, into materials corresponding with those of the brain.

Magnified drawings, executed by Mr. Bauer, of the various substances described in this paper. accompanied the communication.

A *Description of a Method of Determining the Direction of the Meridian*, &c., by J. Pond, Esq. A.R., was also read at this meeting.

The process here described, which Mr. Pond stated to have been tried by him only with instruments of small dimensions, but to be susceptible of great precision, consists in placing two well-defined objects very nearly equi-distant in azimuth from the north meridian line, and distant from each other in azimuth by very nearly twice the azimuth of the pole-star, at its greatest elongation. These objects must be extremely distinct, and well-defined, and the method of placing them is by the use of a

telescope fixed like a transit on a horizontal axis, and pointed first to the pole-star, at its greatest elongation, and then at its reflected image. In thus passing from the star to its image, the central wire, which must necessarily describe a strictly vertical circle, will pass over some terrestrial object: this, if sufficiently well-defined, will serve for the mark; if not, a mark must be erected so as to be bisected, or nearly bisected, by the wire.

The same being done on the other side, the distances of the marks from the central wire must be measured with a micrometer, and this must be repeated, till sufficient exactness is obtained. The horizontal angle between the signals is then to be measured with a theodolite, and in the middle between them a meridian mark erected, and its horizontal angle from each of the signals also measured.

Mr. Pond suggested that this method might be applied to investigations more strictly astronomical; such as determining the places of circumpolar-stars, by their azimuth at their greatest elongation; a method independent of refraction, and which is peculiarly adapted to observatories near the equator, should lateral refraction and undulations of the atmosphere not prove an obstacle.

June 9.—Charles M. Clarke, Esq. was elected a Fellow of the Society, and the following gentlemen were elected foreign members:

Count Chaptal
M. Encke
M. Fresnel
M. Brogniart
M. Bessel.

—— A paper, entitled *Further Researches on the Preservation of Metals by Electro-Chemical Means*, by Sir Humphry Davy, Bart., P.R.S., was read.

After adverting to the general details respecting the protection of the copper sheathing of ships, contained in his former

papers, the President proceeds, in the present communication, to consider the circumstances under which various substances are deposited upon the protecting copper, and their general influence upon its wear, more especially in regard to ships in motion. For this purpose, he availed himself of the use of a steam-boat, employed on an expedition to ascertain some points of longigitude in the north seas, and his inquiries lead to the inference that motion does not affect the nature of the limits and quantity of the protecting metal, and that independent of the chemical, there is likewise a mechanical, wear of the copper, in sailing.

In examining the results of some of the experiments upon the effects of single masses of protecting metal on the sheathing, the author observed, that in same cases, the corrosion seemed to increase with the distance from the protecting metal. It became, therefore, necessary to investigate this circumstance, and to ascertain the extent of the diminution of electrical action, in instances of imperfect or irregular conducting surfaces. Sir Humphry detailed several curious and important experiments in illustration of this inquiry, which prove that any diminution of protecting effect, at a distance, does not depend upon the surface of the metallic, but of the imperfect or fluid, conductor.

His experiments upon perfect and imperfect conductors, led him to another inquiry, important in its practical relations, respecting the nature of the contact between the copper and the preserving metal. He found the protecting action prevented by the thinnest stratum of air, or the finest leaf of talc, or dry paper; but the ordinary coating of rust, or a thin piece of moistened paper, did not impair it

After some experimental details, respecting the electro-chemical powers of metals in solutions excluded from air, Sir Humphry concluded his paper with practical inferences and theoretical elucidations, arising out of its general details. Finding that in certain cases of imperfect connexion, the influence of the protector was weakened by distance, the author proposed that when ships with old sheathing were to be protected, a greater proportion of iron should be used, and, if possible, more distri-

buted. The advantage of this plan was surprisingly shewn in the Samarang, which had been coppered in India, in the year 1821, and came into dock in the spring of 1824, covered with rust, weeds, and zoophytes; she was protected by four masses of iron, equal in surface to about $\frac{1}{80}$ of the copper, two of which were near the stern, and two on the bows. She made a voyage to Nova Scotia, and returned in January, 1825, not, as was falsely reported, covered with weeds and barnacles, but remarkably clean, and in good condition. After citing other instances of the perfect efficacy of the protectors, and adverting to the relative proportion which, in different circumstances, they ought to bear to the sheathing of the vessel, and to the most advantageous methods of applying them, the President concluded by observing upon the importance of selecting perfectly pure copper for the sheathing, of applying it smoothly and equably, and of using, for its attachment, nails of pure copper, and not of mixed metal.

June 16. A Paper was communicated to the Society, *On some New Compounds of Carbon and Hydrogen, and on certain other Products obtained during the Decomposition of Oil by Heat,* by Mr. M. Faraday, F.R.S.

The experiments, of which the results are detailed in this paper, were made principally on the fluid, which is found to be deposited in considerable quantity, when oil gas is compressed. This fluid, as obtained at the works of the Portable Oil Gas Company, is colourless, of a specific gravity less than that of water, insoluble in water except in very minute quantities, soluble in alcohol, ether, oils, *&c.*, and combustible, burning with a dense flame. It is strikingly distinguished from the oil from which it originated, by not being acted upon to any extent by solutions of the alkalies.

Part of this fluid is very volatile, causing the appearance of ebullition at temperatures of 50° or 60°. Other parts are more fixed, requiring even 250° or above for ebullition. By repeated distillations, a series of products were obtained from the mos

to the least volatile, the most abundant being such as occurred from 170° to 200°. On subjecting these, after numerous rectifications, to a low temperature*, it was found that some of them concreted into a crystalline mass, and, ultimately, a substance was obtained from them, principally by pressure at low temperatures, which upon examination, proved to be a new compound of carbon and hydrogen. At common temperatures it appears as a colourless transparent liquid, of specific gravity 0.85 at 60°., having the general odour of oil gas. Below 42° it is a solid body, forming dendritical transparent crystals, and contracting much during its congelation. At 0° it appears as a white or transparent substance, brittle, pulverulent, and of the hardness nearly of loaf sugar. It evaporates entirely in the air: when raised to 186° it boils, furnishing a vapour, which has a specific gravity of 40 nearly, compared to hydrogen as 1. At a higher temperature the vapour is decomposed, depositing carbon. The substance is combustible, liberating charcoal, if oxygen be not abundantly present. Potasium exerts no action upon it below 186°.

This substance was analyzed by being passed over red hot oxide of copper, and by detonation of its vapour with oxygen. The results obtained were, that it consists of

2 proportionals of Carbon	- - -	12
1 ———————— Hydrogen	- -	1
		13

and, that in the state of vapour, six proportionals of carbon and three of hydrogen are present to form 1 volume, which is consequently of the specific gravity of 39, hydrogen being 1. It is named in the paper *Bi-carburet of Hydrogen.*

Experimenting with the most volatile portions of the liquid, a product was obtained, which, though gaseous at common temperatures, condensed into a liquid at 0°. This was found to be very constant in composition and properties. It was very combustible. It had a specific gravity of 27 or 28 as a gas; as a liquid that of 0.627, being the lightest substance, not a gas or vapour, known. When analyzed, it was found to consist of one

proportional of carbon 6., and one of hydrogen 1., as is the case with olefiant gas; but these are so combined and condensed, as to occupy only one half the volume they do in that substance. A volume therefore of the gas contains four proportionals of carbon 24, and four of hydrogen 4=28, which is its specific gravity.

Beside the remarkable difference thus established between this substance and olefiant gas, it is also distinguished by the action of chlorine, which forms with it a fluid body, having a sweet taste, and resembling hydro-chloride of carbon; but from which a chloride of carbon cannot be obtained by the further action of chlorine and light.

The other products from the original fluid, do not present any characters so definite as the above substances; at the same time they appear to be very constant, boiling uniformly at one temperature. They cannot be separated by distillation into more and less volatile parts, so as to afford means of reducing their number to two or three particular bodies. They have the general properties of the original fluid, and with the other products, are all peculiarly acted upon by sulphuric acid, offering phenomena, in the investigation of which the author is at present engaged.

With reference to the presence of these substances in the state of vapour in oil and coal gas, the means of ascertaining it, and the quantity, are pointed out, in the peculiar action of sulphuric acid, causing their perfect condensation, and in the solvent powers over them possessed by fixed and volatile oils, *&c.*, the requisite precautions for their proper application being described. Oil gas was found to be saturated with many of these vapours. Coal gas also contained a portion of them.

The paper concluded with a short reference to the probable uses of the fluid, as originally obtained. If put into gas burning with a blue flame, it makes it produce a bright white flame. It is an excellent solvent of caoutchouc; it will answer all the purposes to which essential oils are applied as solvents; and, having applied that portion of it, which though at common temperatures a liquid at a pressure of 2 or 3 atmospheres, is a gas under any diminished pressure, as fuel to a lamp; the author has shewn

the possibility of such an application, if at any time, such knowledge and command of the decomposition of oil or coal by heat should be obtained, as would enable us to furnish the substance in abundance.

—— An *Account of the Repetition of* M. Arago's *Experiments on the Magnetism developed during the Act of Rotation*, by Chas. Babbage, Esq., F.R.S., and J. F. Herschell, Esq., Sec. R.S., was read.

The experiments of M. Arago having excited much interest, the authors of this communication were induced to erect an apparatus for their verification; and after a few trials, they succeeded in causing a compass to deviate from the magnetic meridian, by setting in rotation under it plates of copper, zinc, lead, *&c.*

To obtain more visible and regular effects, however, they found it necessary to reverse the experiment, by setting in rotation a powerful horse-shoe magnet, and suspending over it the various metals, and other substances to be examined, which were found to follow with various degrees of readiness the motion of the magnet. The substances in which they succeeded in developing signs of magnetism were, copper, zinc, silver, tin, lead, antimony, mercury, gold, bismuth, and carbon in that peculiar metalloidal state in which it is precipitated from carbonated hydrogen in gas works. In the case of mercury, the rigorous absence of iron was secured. In other bodies, such as sulphuric acid, rosin, glass, and other non-conductors, or imperfect conductors of electricity, no positive evidence of magnetism was obtained.

The comparative intensities of action of these bodies were next numerically determined, by two different methods, *viz.*, by observing the deviation of the compass over revolving plates of great size cast to one pattern, and by the times of rotation of a neutralized system of magnets suspended over them, and it is curious that the two methods, though they assigned the same order to the remaining bodies, uniformly gave opposite results in the cases of zinc and copper, placing them constantly above or below each other according to the mode of observation employed.

Our authors next investigated the effect of solution of continuity

on the various metals, in the course of which M. Arago's results of the diminution of effect by division of the metallic plates used were verified; and the further curious fact ascertained, that re-establishing the metallic contact with other metals restores the force, either wholly or in great measure; and that even when the metal used for soldering has, in itself, but a very feeble magnetic power, thus affording a power of magnifying weak degrees of magnetism. The law of diminution of the force by increase of distance was next investigated. It appears to follow no constant progression according to a fixed power of the distance, but to vary between the square and the cube.

The remainder of this paper was devoted to some able and elaborate reasoning on the facts detailed.—The authors conceive that they may be all explained without any new hypothesis in magnetism, by supposing simply that time is requisite both for the developement and loss of magnetism; and that different metals differ in respect, not only of the time they require, but in the intensity of the force ultimately producible in them; and they apply this explanation not only to their own results, but to those obtained by Mr. Barlow in his paper on the rotation of iron.

—— A paper was read containing an account of *Experiments on the Magnetism produced by Rotation*, by S. H. Christie, Esq., in a letter to Mr. Herschell.

Mr. Christie, in this communication, gave an account of some experiments on the developement of magnetism in copper by rotation. He corroborated by his own experience the results obtained by Mr. Herschell, in which a disc of copper was set in rotation by the rotation of one or more magnets beneath it, both in the case where poles of the same name were immediately below the disc, and when of a contrary name. The actions appeared equally intense in both cases, and from this circumstance, he concludes the magnetism thus communicated to the copper, to be extremely transient. The experiment was varied by combining the revolving magnets differently, and the results were stated.

The next experiments of Mr. Christie were directed to the de-

termination of the law according to which the force diminishes as the distance between the disc and magnets increases. It seems to follow from these experiments, that when a thick copper plate is made to revolve under a small magnet, the force tending to deviate the needle is directly as the velocity, and inversely as the fourth power of the distance; but that when magnets of considerable size are made to revolve under their copper discs, the diminution follows more nearly the ratio of the inverse square of the distance, or between the square and the cube, though not in any constant ratio of an exact power.

The author then investigated the law of force when copper discs of different weights are set in rotation which, for small distances, appear proportioned to the weights of the discs, but for smaller ones appear to vary in some higher ratio.

—— A paper *On the Annual Variation of some of the Principal Fixed Stars*, was communicated by John Pond, Esq., A.R., & F.R.S.

This communication consisted principally of a table, stating the annual variations of twenty-three of the principal fixed stars, as deduced from Dr. Brinkley's observations, and those of the Astronomer Royal. On these, Mr. Pond remarks that out of sixteen stars observed at Dublin, thirteen either indicate a southern deviation, or at least are not inconsistent with it; and that of these thirteen, about half indicate a greater deviation than that assigned by Mr. Pond himself; the other half a less; while the three remaining stars deviate northwards.

Mr. Pond further remarked, that the examination of this table is calculated rather to increase than to diminish scepticism on the subject of the determination of such very small quantities, by astronomical observation. He concluded by disclaiming all intention of placing the subject in a controversial point of view, and by expressing a hope that the difficulty will, in a very few years, be satisfactorily cleared up.

—— A *Description of an Improved Hygrometer*, by Mr. Thomas Jones, was read to the Society.

The principle of Mr. Jones's Hygrometer is essentially the same with that of Mr. Daniell's, *viz.*, to ascertain the temperature at which dew is deposited from the atmosphere. It differs from Mr. Daniell's, however, in the frigorific action being applied immediately to the bulb of the thermometer employed to measure the temperature.

This bulb is of considerable size, and of a cylindrical form, slightly flattened and extended at the end. The stem of the thermometer being twice bent at right angles; this end of the bulb turns upwards. It is made of black glass, and is exposed, but the rest of the bulb is covered with muslin. This being moistened with ether, the mercury is cooled, and dew at length settles on the exposed part, at which moment it is read off.

Mr. Jones, after describing this instrument, alludes to an objection to its use, drawn from the application of the frigorific process to the *lower* part of the bulb, while the dew is deposited at the upper. This objection, if realized, might be obviated by inclining the bulb, so as to have its axis horizontal. But repeated trials have satisfied him of there being no occasion for this precaution.

—— A paper *On the Nature of the Functions of Mortality, and on a New Mode of determining the Value of Life Contingencies*, was communicated by Benjamin Gompertz, Esq., F.R.S.

This paper, which is a continuation of former researches on the same subject printed in the Transactions of the Royal Society, was divided into two chapters. In the first the author considers the nature of the law of those numbers in tables of mortality which express the amount of persons living at the end of ages in regular arithmetical progression. He remarks first, that for short intervals the law approaches nearly to a decreasing geometrical progression, and that this must be the case, whatever be the strict expression for the law of mortality, provided the intervals do not exceed certain limits. But he further remarks that this property will be found to belong to very ex-

tensive portions of tables of mortality, and instances Deparcieux's tables, where from the age of 25 to that of 45, the numbers living at the end of each year decrease very nearly in geometrical progression.

Considering however the whole extent of such a table, it will be found that the ratio of this geometrical progression is not the same in all parts of the table. But before he entered on this consideration, the author drew some consequences from the hypothesis of a geometrical progression being the strict law of nature after a certain age. One of these is the equality of value of all life annuities commencing after that age. Another is, that the want of instances in history of persons living to very enormous ages (waving those of the patriarchs), is no proof that such may not be the law of nature, as he shews by calculation, that out of 3,000,000 persons of 92, not more than one should on this supposition reach 192. This leads him to some general considerations on the causes of death, after which he resumes the consideration of the general law of the tables.

To find this *à priori*, he supposes that a person's resistance to death decreases as his years increase, in such a manner that he in every equal infinitely small portion, loses equal infinitely small portions of his vital powers. He further supposes, that among any given number of persons of equal vital powers the probability of death is the same, but that among all, it inversely is proportional to the vitality. These postulata being assumed, he enters into an analytical investigation, the result of which is the representation of the Law of Life by such a function as is sometimes called a double exponential, that is geometric progression in which the ratio is itself variable in geometric progression.

He then proceeds to examine the coincidence of this law with several tables of the best authority, such as those of Deparcieux, Northampton, the Swedish and Carlisle tables, and the supposed experience of the Equitable Assurance Office. The results of their comparisons are stated in a tabular form, and are very favourable to the law supposed.

In the second chapter, the author, after briefly explaining by algebraical reasoning, the mode of applying true principles to calculations of annuities, proposes general tables for facilitating this application in practice. These tables (which occupy 28 folio pages, and represent the logarithms of the present values of annuities for every value of a certain argument), are actually calculated, and annexed to the communication, forming the principal part of the second chapter.

Art. XIV. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. XXII.

i. *Further Examination of Captain* Thomson's *Tables. By a* Correspondent.

It is thought right to submit to the public some further results obtained from Captain Thomson's method of clearing the *Lunar Distance*, the method of the Appendix to the *Requisite Tables*, and Mr. Lax's method; the examples before computed not being sufficiently diversified to ascertain the comparative accuracy of those methods in all cases.

	°	′	″	Dist. computed by Capt. Thomson's method. ° ′ ″	Dist. computed by the method in the App. to Req. Tables. ° ′ ″	Dist. computed by Mr Lax's method. ° ′ ″
✳'s app. alt.	6	0	0			
☽'s . . .	8	0	0	20 6 36½	20 6 44½	20 6 45
App. dist.	20	0	0			
☽'s hor. par.		61	0			
✳'s app. alt.	16	0	0			
☽'s . . .	14	0	0	19 53 57	19 53 49½	19 53 49½
App. dist.	20	0	0			
☽'s hor. par.		54	0			
✳'s app. alt.	16	0	0			
☽'s . . .	14	0	0	19 52 58	19 53 7½	19 53 6½
App. dist.	20	0	0			
☽'s hor. par.		61	0			

	°	′	″	Dist. computed by Capt. Thomson's method.	Dist. computed by the method in the App. to Req. Tables.	Dist. computed by Mr. Lax's method.
✳'s app. alt.	15	0	0			
☽'s . .	18	0	0	22° 6′ 4$\frac{3}{4}$″	22° 6′ 13″	22° 6′ 14$\frac{1}{2}$″
App. dist.	22	0	0			
☽'s hor. par.		61	20			
✳'s app. alt.	13	0	0			
☽'s . .	18	0	0	20 1 59	20 1 48	20 1 48
App. dist.	20	0	0			
☽'s hor. par.		54	0			
✳'s app. alt.	13	0	0			
☽'s . .	14	0	0	20 2 2	20 2 12	20 2 12$\frac{1}{2}$
App. dist.	20	0	0			
☽'s hor. par.		61	20			
✳'s app. alt.	7	35	28			
☽'s . . .	7	55	31	29 51 16	23 51 0	23 51 0
App. dist.	23	50	27			
☽'s hor. par.		54	0			

The small difference, which is sometimes found between Mr. Lax's results and those of the App. to the Req. Tables, arises chiefly from the circumstance of the moon's parallax in altitude having been computed by Mr. Lax from the apparent altitude diminished by the refraction, instead of the apparent altitude itself, as is done in the Req. Tables.

In the following Example all the figures are written down, which must necessarily be written down in the operation itself, in order that the length of it in each method may be fairly exhibited.

	°	′	″
☉'s app. alt.	41	47	26
☽'s	9	54	34
App. dist.	43	37	12
☽'s hor. par.		547	

Captain Thomson's Method.

	°	′	″					
☽'s hor. par.	0	54	7	Log.	0.0619	. . . .	Log.	0.0619
☉'s app. alt.	41	47	26	Log.	0.6363	☽'s app. alt. 9° 54′ 34″	Log.	1.2239
App. dist.	43	37	12	Log.	0.8387	. . . .	Log.	0.9790
First corr.	4	7	43	Log.	1.5369			
Second corr.	5	9	47	. . .	. . .	. . . .	Log.	2.2648
Third corr.		3	48.1					
True dist.	42	58	30.1					

Diff. of third corr. for 4° ☉'s alt. + 24″
——— 1 ☽'s alt. − 28
——— 4 dist. − 11

$4^{\circ} : 3' \, 37'' :: + 24''$
60 60
240 217
24
868
434
240) 5208 (21.7
480
408
240
1680

$60' : 55' :: - 28''$
28
440
110
60) 1540
25.7

Hence 3′ 58″
+ 21.7
− 25.7
− 9.9
Tab. P. + 4
Third corr. 3 48.1

$4^{\circ} : 3' \, 37'' :: - 11''$
60 60
240 217
11
240) 2387 (9.9
2160
2270

Method in the Appendix to Req. Tables.

	°	′	″			
☉'s app. alt.	41	47		☉'s corr.	0° 56½′	} 48′ 57⅓″
☽'s ———	9	54	34	☽'s ——	48 1	
Diff. app. alt.	31	52	52	Vers.	150721	Corrd. L. T. IX. 9.999003
					133	
					150854	
App. dist.	43	37	2	Vers.	276029	
					40	
					276069	
				Diff.	125215	. . . . . Log. 5.097604
						51
				No.	124928	. . . . . . 5.096655
Diff. true alt.	31	3	54½	Vers.	143282	
					139	
True dist.	42	58	30	Vers.	268349	

Diff. of ☽'s corr. for 1° alt. + 24″
——————— 10′ par. + 10

$60' : 55' :: +24''$
24
220
110
60) 1320

For mins. alt.	+ 22″
For secs. par.	+ 7
	47 32
☽'s cor.	48 1

Diff. of Log. T. IX. for 1° alt. − 117
——————— 10′ par. − 4

$60' : 55' :: -117''$
55
585
585
60) 6435

For mins. alt.	− 107.2
For secs. par.	− 3
	9.999126
	9.999016
Tab. X.	− 13
Corrd. L. T. IX.	9.999003

Mr. Lax's Method.

☉'s app. alt.	41° 47′ 26″			
☽'s ——	9 54 34			
☉'s corr.	0 57			
☽'s ——	47 53	Log. 9.998959		
	2	8		
	7	19		
Sum	10 43 33	15		
App. dist.	43 37 12		Ver.	276029
				40
Diff. app. alt.	31 52 52		Suv.	1849126
				20
		Log. 5.097608	No.	125215
		51		
		Log. 5.096660	No.	124930
Diff. true alt^s.	31 3 53		Ver.	143282
				134
True dist.	42 58 29		Ver.	268346

Statement of the different openings of the book, the operations performed, the number of figures employed, and the number of pages in the Tables used, in working out the above example by each method.

	Openings of the Book	Operations performed	Openings and Operations	Figures employed	Pages in the Tables used
Capt. Thomson	7	7	14	207	87
App. Req. Tables	7	13	20	213	106
Mr. Lax . . .	5	6	11	121	81

In the second and third methods there is only one opening of the book in taking out the numbers for the difference between the apparent altitudes, and for the difference between the true altitudes; and also for the apparent and true distances; and in

taking out the logarithms and the natural number; and accordingly only three openings are put down on these accounts in the above statement. There will always be the same number of openings of the book, and of pages in the tables used, in these two methods, and very nearly the same number of figures to be written down. In Captain Thomson's method likewise there will always be the same number of openings of the book, and of pages in the tables used; but in general there will be fewer operations, and of course fewer figures to be written down, than in the above example; because whenever the difference between the two successive corrections in Table XVIII. is very small, the proportional part may be taken out by inspection. But if two of them could be thus satisfactorily taken out, and only one proportion were required to be written down, still the operation would not be shorter than in other methods. It may be remarked, too, that although an opening of the book is saved in clearing the distance by including the refraction of the two objects in the quantity taken from Table XVIII, this correction must afterwards be made in deducing the time from the observed altitude of one of them; so that, in fact, the openings ought to be considered as eight instead of seven, as stated above. The moon's altitude indeed is evidently not intended to be used for this purpose, which is certainly a great defect, as the altitude of the moon will frequently be preferable to that of the other object, and will very often be the only one that can be employed. No table, however, is given of its corrections; and of course, if its altitude were to be used in finding the time, the correction must either be taken from another book, or computed by a troublesome process. It may be observed, likewise, that the combining together of positive and negative quantities in the third correction must be embarrassing in practice, and may sometimes be the occasion of error. It is, moreover, unpleasant to have to subtract the correction for one distance from that of another when they lie on different sides of a leaf, as is always the case in Table XVIII. A further objection is, that the method does not apply to cases in which the apparent distance is less than 20°, though not unfrequently the distance between the moon and star is much less in

the N. Almanac [?]. Neither is it obvious how this method is to be used when the difference between the given apparent altitudes of the two objects is greater than the apparent distance in Table XVIII, next less than the given apparent distance; as for instance, when the star's apparent altitude is 54°, the moon's 15°, and the apparent distance 39° 30′. Lastly, it is a serious defect in this method that no means are pointed out of applying a correction for any extraordinary change in the state of the atmosphere, nor any satisfactory rule given for estimating the effect of the unequal refraction at the upper and lower limbs of the sun and moon, when observed near the horizon. These, however, are considerations of too much importance to be altogether neglected in the present state of Nautical Astronomy. By taking them into the account in the example at page 282 of Mr. Lax's book, the longitude is made to differ more than 23 miles from the longitude deduced in the same example at page 273, where they are neglected.

[It is, however, but fair to remark, that the finding the differences of the corrections for fractional parts in this, as in almost all other cases, will be much more expeditiously done by the arithmetical rule of *Practice* than by the *Rule of Three.* Thus in the proportion 4° : 3′ 37″ :: + 24′, it is easy to perceive that $24' = \frac{1}{10}$, and to take at once 21″.7 as the result. When the parallax is very great or very small, the correction by Captain Thomson's method must confessedly be defective.]

ii. *Tables of Third and Fourth* Differences, *for interpolating the Moon's place. By* Thomas Henderson, Esq.

In calculating the moon's place by interpolation from the Nautical Almanac, especially in right ascension and declination, when great precision is required, the equation of mean second difference alone will not be sufficient, but the third, fourth, and fifth differences must be taken into account, as the corrections arising from them may sometimes amount to 3 or 4 seconds. The two small Tables annexed are designed to give these corrections, and are to be used thus:

Take out of the Ephemeris the three right ascensions and pre-

preceding, and the three following the given time, and take their first, second, third, and fourth differences, also the mean of the two second differences standing on each side of the given time, and the mean of the two fourth differences. Then to the proportional part of the middle first difference, corrected by the equation of mean second difference, apply the correction found in the first Table annexed answering to the middle third difference, and the correction in the second Table answering to the mean fourth difference, and the result will be the moon's place correct.

Examples.

1. Given the following right ascensions of the moon from the Nautical Almanac, required her right ascension on 6th July, 1823, at 3 hours.

1823		A. R.	1st diff.	2d diff.	3d diff.	4th diff.	Mean
		° ′ ″	° ′ ″	′ ″	′ ″	″	″
July 5,	N.	60 30 55	+ 8 7 58	+ 10 42	− 4 50	− 38	− 18¼
	M.	68 38 53	8 18 40	5 52	5 28	+ 1	
6,	N.	76 57 33	8 24 32	0 24	5 27		
	M.	85 22 5	8 24 56	− 5 3			
7,	N.	93 47 1	8 19 53	Mean			
	M.	102 6 54		+ 3 8			

	°	′	″
Moon's A. R. July 6, Noon . . .	76	57	33
Proportional part of first diff. . . .	2	6	8
Equation of mean second diff. . .			− 17·6
Equation of third diff.			− 2·5
Equation of fourth diff.			− 0·3
Moon's A. R. July 6, at 3 hours . . .	79	3	20·6

2. Required the moon's right ascension on 4th July, 1823, at 18 hours.

1823		A. R.	1st diff.	2d diff.	3d diff.	4th diff.	Mean
		° ′ ″	° ′ ″	′ ″	′ ″	′ ″	′ ″
July 3,	M.	37 43 7	+ 7 18 2	+ 18 18	− 1 12	− 1 22	− 1 19
4,	N.	45 1 9	7 36 20	17 6	2 24	1 16	
	M.	52 37 29	7 53 26	14 32	3 50		
5,	N.	60 30 55	8 7 58	10 42			
	M.	68 38 53	8 18 40	Mean			
6,	N.	76 57 33		15 49			

	°	′	″
Moon's A. R. July 4th, Midnight . . .	52	37	29
Proportional part of first diff. . . .	3	56	43
Equation of mean second diff. . . .		− 1	58·7
Equation of third diff.			0·0
Equation of fourth diff. . . .			− 1·9
Moon's A. R. July 5th, at 18 hours . .	56	32	11·4

The formulæ from which the Tables were constructed are thus investigated.

Let a be the first of the series of six terms given in the Ephemeris, and d^1, d^2, d^3, d^4 and d^5, be the first terms of the several orders of differences.

(A.)

Then by the usual formula of interpolation, the value of any term of the series at the distance x from the first term a is $=$

$$a + xd^1 + x \times \frac{x-1}{2} d^2 + x \times \frac{x-1}{2} \times \frac{x-2}{3} d^3 + x \times \frac{x-1}{2} \times \frac{x-2}{3} \times \frac{x-3}{4} d^4 + x \times \frac{x-1}{2} \times \frac{x-2}{3} \times \frac{x-3}{4} \times \frac{x-4}{5} d$$

$$= a + xd^1 + (x^2 - x)\frac{d^2}{2} + (x^3 - 3x^2 + 2x)\frac{d^3}{6} + (x^4 - 6x^3 + 11x^2 - 6x)\frac{d^4}{24} + (x^5 - 10x^4 + 35x^3 - 50x + 24x^2)\frac{d^5}{120}.$$

(B.)

Now the term to be interpolated being between the third and fourth terms of the series, let y be its distance from the third term, then $x = y + 2$, which being substituted in the above formula it becomes $a + (y+2)d^1 + (y^2 + 3y + 2)\frac{d^2}{2} + (y^3 + 3y^2 + 2y)$

$$\frac{d^3}{6} + (y^4 + 2y^3 - y^2 - 2y)\frac{d^4}{24} + (y^5 - 5y^3 + 4y)\frac{d^5}{120}.$$

The fifth difference being d^5, the mean of the two fourth differences will be $= d^4 + \frac{d^5}{2} = D^4$ the third difference standing opposite to the interval between the third and fourth terms will be $= d^3 + d^4 = D^3$ the mean of the two second differences oppo-

site to the third and fourth terms will be $d^2 + \frac{3}{2} d^3 + \frac{1}{2} d^4 = D^2$ the first difference opposite to the same interval will be $= d^1 + 2d^2 + d^3 = D^1$, and the third term of the series will be $= a + 2d^1 + d^2 = c$.

Hence

$$d^4 = D^4 - \frac{1}{2} d^5$$

$$d^3 = D^3 - d^4 = D^3 - D^4 + \frac{1}{2} d^5$$

$$d^2 = D^2 - \frac{3}{2} d^3 - \frac{1}{2} d^4 = D^2 - \frac{3}{2} D^3 + D^4 - \frac{1}{2} d^5$$

$$d^1 = D^1 - 2d^2 - d^3 = D' - 2D^2 + 2D^3 - D^4 + \frac{1}{2} d^5$$

$$a = c - 2d^1 - d^2 = c - 2D^1 + 3D^2 - \frac{5}{2} D^3 + D^4 - \frac{1}{2} d^5$$

And substituting these values of d^4, &c. in formula (B) it becomes

$$c + y\,D^1 + (y^2 - y)\frac{D^2}{2} + (2y^3 - 3y^2 + y)\frac{D^3}{12} + (y^4 - 2y^3 - y^2 + 2y)\frac{D^4}{24} + (2y^5 - 5y^4 + 5y^2 - 2y)\frac{d^5}{240}.$$

Now yD^1 is the simple proportional part of the middle first difference; $(y^2 - y)\frac{D^2}{2} = D^2 \times y \times \frac{y-1}{2}$ is the equation of mean second difference; $(2y^3 - 3y^2 + y)\frac{D^3}{12}$ is the equation of third difference in the annexed Table 1; $(y^4 - 2y^3 - y^2 + 2y)\frac{D^4}{24}$ is the equation of mean fourth difference in Table II; and the remaining term $(2y^5 - 5y^4 + 5y^2 - 2y)\frac{d^5}{240}$ is neglected, as at its maximum it can only amount to $\frac{1}{1152}$ of the fifth difference, and is therefore insensible. Hence applying all these equations, we have the value of the term required, the same as if the common formula of interpolation had been employed.

TABLE I.

Equation of Third Difference.

Time after Noon or Midnight.	THIRD DIFFERENCE.															Time after Noon or Midnight.
	′ ″ 1 0	′ ″ 2 0	′ ″ 3 0	′ ″ 4 0	′ ″ 5 0	′ ″ 6 0	′ ″ 7 0	′ ″ 8 0	′ ″ 9 0	′ ″ 10 0	′ ″ 0 10	′ ″ 0 20	′ ″ 0 30	′ ″ 0 40	′ ″ 0 50	
+																−
H. M.	″	″	″	″	″	″	″	″	″	″	″	″	″	″	″	H. M.
0 0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	12 0
0 30	0.2	0·4	0·5	0·7	0·9	1·1	1·3	1·5	1·6	1·8	0·0	0·1	0·1	0·1	0·2	11 30
1 0	0·3	0·6	1·0	1·3	1·6	1·8	2·2	2·5	2·9	3·2	0·1	0·1	0·2	0·2	0·3	11 0
1 30	0·4	0·8	1·2	1·6	2·1	2·5	2·9	3·3	3·7	4·1	0·1	0·1	0·2	0·3	0·3	10 30
2 0	0·5	0·9	1·4	1·9	2·3	2·8	3·2	3·7	4·2	4·6	0·1	0·2	0·2	0·3	0·4	10 0
2 30	0·5	1·0	1·4	1·9	2·4	2·9	3·4	3·8	4·3	4·8	0·1	0·2	0·2	0·3	0·4	9 30
3 0	0·5	0·9	1·4	1·9	2·3	2·8	3·3	3·7	4·2	4·7	0·1	0·2	0·2	0·3	0·4	9 0
3 30	0·4	0·9	1·3	1·7	2·2	2 6	3·0	3·4	3·9	4·3	0·1	0·1	0·2	0·3	0·4	8 30
4 0	0·4	0·7	1·1	1·5	1·9	2·2	2.6	3·0	3·3	3·7	0·1	0·1	0·2	0·2	0·3	8 0
4 30	0·3	0·6	0·9	1·2	1·5	1·8	2·1	2·3	2·6	2·9	0·0	0·1	0·1	0·2	0·2	7 30
5 0	0·2	0·4	0·6	0·8	1·0	1·2	1·4	1·6	1·8	2·0	0·0	0·1	0·1	0·1	0·2	7 0
5 30	0·1	0·2	0·3	0·4	0·5	0.6	0·7	0·8	0·9	1·0	0·0	0·0	0·1	0·1	0·1	6 30
6 0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	0·0	6 0
+																−

Third difference being positive, and hour from noon or midnight less than 6 hours, the correction is positive, but hour being more than 6 hours, correction is negative.
Third difference being negative, the reverse.

TABLE II.

Equation of Fourth Difference.

Time after Noon or Midnight.	Fourth Difference.										Time after Noon or Midnight.
	1′ 0″	2′ 0″	3′ 0″	4′ 0″	5′ 0″	0′ 10″	0′ 20″	0′ 30″	0′ 40″	0′ 50″	
H. M.											H. M.
0 0	″0·0	″0·0	″0·0	″0·0	″0·0	″0·0	″0·0	″0·0	″0·0	″0·0	12 0
0 30	0·2	0·4	0·6	0·8	1·0	0·0	0·1	0·1	0·1	0·2	11 30
1 0	0·4	0·8	1·2	1·6	2·0	0·1	0·1	0·2	0·3	0·3	11 0
1 30	0·6	1·2	1·7	2·3	2·9	0·1	0·2	0·3	0·4	0·5	10 30
2 0	0·7	1·5	2·2	3·0	3·7	0·1	0·2	0·4	0·5	0·6	10 0
2 30	0·9	1·8	2·7	3·6	4·5	0·1	0·3	0·4	0·6	0·7	9 30
3 0	1·0	2·1	3·1	4·1	5·1	0·2	0·3	0·5	0·7	0·9	9 0
3 30	1·1	2·3	3·4	4·6	5·7	0·2	0·4	0·6	0·8	0·9	8 30
4 0	1·2	2·5	3·7	4·9	6·2	0·2	0·4	0·6	0·8	1·0	8 0
4 30	1·3	2 6	3·9	5·2	6·5	0·2	0·4	0·7	0·9	1·1	7 30
5 0	1·4	2·7	4·1	5·4	6·8	0·2	0·5	0·7	0·9	1·1	7 0
5 30	1·4	2·8	4·2	5·6	7·0	0·2	0·5	0·7	0·9	1·2	6 30
6 0	1·4	2·8	4·2	5·6	7·0	0·2	0·5	0·7	0·9	1·2	6 0

Equation is of the same sign as the Fourth Difference.

iii. *Corrections of the Catalogue of* ZODIACAL STARS *inserted in the Third Number of the Astronomical Collections.* By THOMAS HENDERSON, Esq.

No. 24, μ ♊ A. R. *for* $6^{h}\ 12^{m}\ 3^{s}.09$ *read* $6^{h}\ 12^{m}\ 4^{s}.09$.

No. 52, κ ♍ N. P. D. *for* 99 24 57.0 *read* 99 25 48.6.

No. 63, 1 β ♏, A. R. *for* 15 54 56.11 *read* 15 54 59.11.

The last is an error of Mr. Pond's Catalogue: it had before been pointed out by Professor Schumacher. Mr. Henderson's remark on Bouvard's computation of the difference of longitude of Greenwich and Paris, published in a former number of these Collections, has also occurred to Professor Slavinski, and has been admitted by Mr. Bouvard as correct.

ART. XV. ANALYSIS OF SCIENTIFIC BOOKS.

I. *Philosophical Transactions of the Royal Society of London, for the Year* 1825. Part II.

The first paper in this Part of the Transactions is entitled,

i. *On the Effects of Temperature on the Intensity of Magnetic Forces, and on the Diurnal Variation of the Terrestrial Magnetic Intensity. By* Sam. Hunter Christie, Esq., M.A. of Trin. Col., Camb., Fel. Camb. Phil. Soc.; of the Royal Military Academy Communicated by the President.

THE results of Mr. Christie's experiments upon the above subjects are given in an extended series of tables, commencing with a temperature of $-3°$ F., and terminating at 127°. From a temperature of 80°, the magnetic intensity decreased rapidly as the temperature increased, and at above 100° a portion of the magnetic power was permanently destroyed. But we shall not follow the author into the details of his researches, in consequence of the singularly fluctuating, yet progressive, state of magnetic science at the present moment; on another occasion we shall enter at length upon the subject, and endeavour to do justice to Mr. Christie's labours.

ii. *The Croonian Lecture. On the Existence of Nerves in the Placenta. By* Sir E. Home, Bart., V.P.R.S.

AFTER complimenting Mr. Bauer on his microscopic infallibility, Sir Everard proceeds to the enunciation of his discovery of placental nerves, a discovery " which he is proud to say was not the result of accident, but of a regularly-arranged plan for that purpose."

That nerves are necessary for other purposes than mere sensation has long been admitted by physiologists, and it has been proved by many well-devised experiments, that the processes of secretion and growth of parts are under their immediate influence. Mr. Bauer first detected them in the placenta of a seal, and afterwards in the transparent portion of the chorion of the tapir, and thence their existence in the human placenta, and a consequent direct nervous communication between the mother and the child was inferred, and afterwards proved. We think, however, that these investigations are not yet sufficiently mature to warrant the inferences that are drawn from them.

iii. *Observations on the Changes the Ovum of the Frog undergoes during the Formation of the Tadpole. By* Sir E. Home, Bart., V.P.R.S.

In the year 1822, the author laid before the Society a series of observations on the progress of the formation of the chick in the egg of the pullet, illustrated by drawings from the pencil of Mr. Bauer, showing that in the ova of hot-blooded animals the first parts formed are the brain and spinal marrow. He has now brought forward a similar series on the progress of organization in the ova of cold-blooded animals, illustrated in the same manner by microscopical drawings made by the same hand.

The ova of the frog, which have been selected for this investigation, are found to have no yelk. If we examine these ova in the ovariā in which they are formed, we find them to consist of small vesicles of a dark colour; when they enter the oviducts they enlarge in size, and acquire a gelatinous covering, which increases in quantity in their course along those tubes; but the ova car neither be said to have acquired their full size nor to have received their proportion of jelly, till they arrive at a cavity close to the termination of each oviduct, formed by a very considerable enlargement of those tubes, corresponding in many respects to the cloaca in which the pullet's egg is retained till the shell becomes hard.

When the ova are deposited in these reservoirs, they become completely formed, and in a state to be inpregnated by the male influence, which is applied to them in the act of their expulsion. As they are pressed upon each other, by being confined in a small space, the gelatinous covering takes an hexagonal figure, in the centre of which is the ovum.

Immediately after inpregnation there is no change in the appearance of the jelly, nor of the vesicle contained in it, in this respect corresponding exactly with what happens to the pullet's egg. The first change that is produced towards the formation of an embryo is, the contents of the vesicle expand, its form changes from that of a sphere to an oval, and when cut through its contents are no longer fluid. In the act of coagulation, the central portion becomes of a lighter colour than that which surrounds it, swells out in the middle, and there is a distinct line by which the two portions are separated from one another; the central part, in its future changes, is converted into brain and spinal marrow, and after these organs have acquired a defined outline, the heart and other viscera are seen forming in the darker substance.

The membrane that forms the vesicle which is destined to contain the embryo when it has become a tadpole, has a power of enlargement as the embryo increases in size, and then performs the office both of the shell and of the membrane that lines it in the

pullet's egg, at the same time serving as a defence to protect it, and allow of the blood being aërated.

The black matter, says Sir Everard, lining the vesicle can only answer some secondary purpose, since it is not met with in the aquatic salamander, whose mode of breeding very closely resembles that of the frog. Upon reflecting that the frog's spawn is exposed to the scorching effect of the sun, and in places where there is no shelter, this nigrum pigmentum may be given to the eggs as a defence for the young during its growth, which cannot be required in those of the aquatic salamander, since they are separately enclosed within the twisted leaves of water-plants, and screened from the full force of the sun's rays. The plant whose leaves the aquatic salamander most generally selects to lay its eggs upon is the polygonum persicaria.

This paper is illustrated by three very curious and instructive plates.

iv. *A general Method of calculating the Angles made by any Planes of Crystals, and the Laws according to which they are formed. By the* Rev. W. Whewell, F.R.S., Fel. Trin. Col. Camb.

The author, after stating the inconveniencies, inelegancies, and imperfections of the received notation for expressing the planes of a crystal, and the laws of decrement by which they arise, and of the usual methods of calculating their angles, explains the object of the present paper, which is, to propose a system exempt from these inconveniencies, and adapted to reduce the mathematical portion of crystallography, to a small number of simple formulæ of universal application. According to the method here followed each plane of a crystal is represented by a symbol indicative of the laws from which it results, which, by varying only its indices, may be made to represent any law whatever; and by means of these indices, and of the primary angles of the substance, we may derive a general formula, expressing the dihedral angle contained between *any one plane* resulting from crystalline laws, and *any other*. In the same manner we can find the angle contained between any two edges of the derived crystal. Conversely having given the plane, or dihedral angles, of any crystal and its primary form, we can, by a direct and general process, deduce the laws of decrement according to which it is constituted.

The purely mathematical part of this paper depends on two formulæ demonstrated by the author elsewhere, and here assumed as known, by means of one of which the dihedral angle included between any two planes can be calculated when the equations of both planes are given, and by the other the plane angle included between any two given right lines can, in like manner, be expressed by assigned functions of the coefficients of their equations supposed given. These formulæ being taken for granted, nothing

remains but to express by algebraical equations the planes which result from any assigned laws of decrement, for the different primitive forms which occur in crystallography.

To this effect the author assumes one of the angles of the primitive form, supposed, in the first case, a rhomboid, as the origin of three co-ordinates, respectively parallel to its edges, and supposes any secondary face to arise from a decrement on this angle, by the subtraction of any number of molecules on each of the three edges. It is demonstrated, first, that the equation of the plane arising from this decrement will be such that the coefficients of the three co-ordinates in it (when reduced to its simplest form) will be the reciprocals of the numbers of molecules, subtracted on the edges to which they correspond. If the constant part of this equation be zero, the face will pass through the origin of the co-ordinates; if not, a face parallel to it may be conceived passing through such origin, and will have the same angles of incidence, *&c.*, on all the other faces of the crystal, so that all our reasonings may be confined to planes passing through the origin of the co-oridnates.

To represent any face, the author encloses between parentheses the reciprocal coefficients of the three co-ordinates of its equation, or rather of the numbers of molecules subtracted on each of the three edges to form it, with semicolons between them. This he calls the symbol of that face. He then shews how truncations on all the different edges and angles of the primitive form are represented in this notation, by one or more of the elements of which the symbol consists becoming zero or negative, thus comprehending all cases which can occur in one uniform analysis.

The law of symmetry in crystallography requires that similar angles and edges of the primitive form should be modified similarly, to form a perfect secondary crystal. This gives rise to *coexistent planes*. In the rhomboid these coexistent planes are found by simple permutations of the elements of the symbol one among another. In the prism, such only must be permuted as relate to similar edges. In other primitive forms, as for example in the tetrahedron, the author institutes a particular inquiry into the decrements of coexistent planes which truncate the different angles of the primitive form, as referred to that particular angle which he assumes as the origin of the co-ordinates. It follows from this analysis that, in this latter case, each of the elements of the symbol must be combined with its excess over each of the remaining two, to form a new symbol. This gives four symbols, including the original one, each susceptible of six permutations, making in all twenty-four faces.

The author then proceeds to consider the cases of the irregular tetrahedron and octohedron, the triangular prism and rhomboidal dodecahedron, investigating in each case the symbols of the coexistent planes, and illustrating his theory with examples taken from

the crystalline forms of zircon, sulphur, and other minerals. He next treats of the order in which the faces lie in a perfect crystal, and the determination of such faces as are adjacent or otherwise. To this end he conceives an ellipsoid inscribed within the crystal, having for its three axes the three most remarkable lines in the primitive form, and by means of the well-known equation of the second degree representing such an ellipsoid, combined with the equation of any proposed face, he deduces the longitude and latitude (on the surface of the ellipsoid) of the point at which it would be touched by a plane parallel to such face. The results are included in general and explicit formulæ, by whose application in any proposed case the sequence and arrangement of the faces in the perfect crystal is readily discovered.

The angles made by edges of the secondary form are next investigated, after which the author, having recapitulated his results, takes occasion to refer to a paper by Mr. Levy, who had previously, but unknown to Mr. Whewell, employed the representation of a secondary plane by its equation referred to the three principal edges of the primitive form, but only in a particular case, whereas the investigations and notation in the present paper are absolutely general.

In the course of this paper Mr. Whewell instances the application of his analysis in the solution of the following problems :—

Knowing the dihedral angles of the secondary rhomboid, to find the symbol of its faces, or their laws of decrement.

To find what laws of decrement give a secondary rhomboid similar to the primary one.

Knowing the lateral angles made by the planes of any bi-pyramidal dodecahedron, to find the symbols.

Knowing the angles made by any plane with two primary planes, to find its symbol.

To find what laws give prisms parallel to the axis of the rhomboid.

To find the symbol of a plane which truncates the edge of any secondary rhomboid.

v. *Explanation of an Optical Deception in the Appearance of the Spokes of a Wheel seen through Vertical Apertures. By* P. M. Roget, M.D., F.R.S.

The optical deception here described is that which people see when they chance to look at a revolving carriage-wheel through the upright bars of a window-blind, or railings, or any similar series of fixed vertical obstacles; in such a case, all the spokes, except such as happen to be perpendicular, appear curved, the convexity being always turned downwards.

A certain degree of velocity in the wheel is necessary to produce this deception, and if this be gradually communicated, the

principal effect of curvature is observed to come on suddenly, but the degree of it is independent of the velocity of the wheel, each image appearing motionless during the moment that it is viewed. The deception is improved by every circumstance which tends to abstract attention from the bars, and fix it on the wheel.

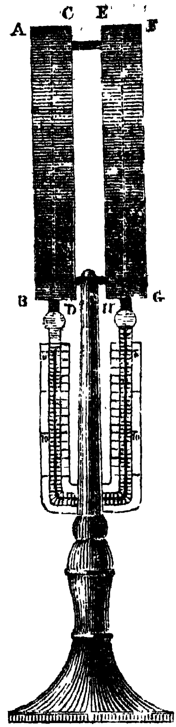
The number of curved images was found to depend upon the ratio of the angles subtended at the eye by the intervals between the bars, and those between the extremities of the spokes, being greater as this ratio was less. The combination of a progressive with a rotatory motion was also found essential to the phenomena.

From these and other circumstances, Dr. Roget refers this deception to the principle that an impression made by a pencil of rays on the retina, will, if sufficiently vivid, remain for a certain time after the cause has been withdrawn. He enters into a lengthy application of this principle to the case before us, and shows that the apparent form of each spoke will be a curve formed by the continual intersection of the revolving and advancing radius with the immoveable interval between the bars referred to the plane of the wheel. The general form of these curves he refers to the class of Quadratrices, and the most remarkable among them is that discovered by Dinostrates, and known by his name.

vi. *On a new Photometer, with its Application to determine the relative Intensities of artificial Light, &c. By* William Ritchie, A. M., *Rector of the Academy at Tain. Communicated by the President.*

This new photometer consists of two cylinders of planished tin plate from 2 to 10 or 12 inches in diameter, and from a quarter of an inch to an inch deep. One end of each cylinder is enclosed by a circular plate of the same metal soldered completely air-tight, the other ends being shut up by circular plates of the finest and thickest plate-glass, made perfectly air-tight. Half way between the plates of glass and the ends of the cylinders, there is a circular piece of black bibulous paper, for the purpose of absorbing the light which permeates the glass, and converting it into heat.

The two cylinders are connected by small pieces of thermometer-tube which keep them steady with their faces parallel to each other, but turned in opposite directions, and also serve to make the insulation as complete as possible. The chambers are then connected by a small bent tube in the form of the letter U, having small bulbs near its upper extremities, and containing a little sulphuric acid, tinged with carmine. The instrument is supported upon a pedestal, having a vertical opening through the stem to allow the glass-tube to pass along it, and thus secure it from accidents. A small scale divided into any number of equal parts, is attached to each branch of the tube.



In the annexed figure, ABCD and EFGH are the cylinders, AB and FG the plates of glass. CD, EFG the ends shut up by the circular tin plates, the blackened paper is represented by the lines between AB, CD and EH, FG. The other parts will be obvious from the mere inspection of the figure.

The accuracy of the instrument evidently depends upon the perfect equality of its two opposite ends. To ascertain, if it be accurately constructed, Mr. Ritchie directs us to place it between two steady flames, and move it nearer the one or the other till the liquid in the tube remains stationary, at the division of the scale at which it formerly stood. Turn it half round without altering its distances from the flames, and if the liquid remains stationary at the same division, the instrument is correct. To show the extreme delicacy of the instrument, he says, that if placed opposite a single candle, it will be sensibly affected at the distance of 10, 20, or 30 feet, provided it be of sufficient diameter, whilst it will not be sensibly acted upon at the same distance by a mass of heated iron affording twenty times the quantity of heat.

Our author then proceeds as follows:—"Place the instrument between any number of steady lights whose intensities are known, as for example, between four wax candles opposite one end, and one candle opposite the other, and move the photometer till the fluid remain stationary at the division where it formerly stood, and it will be found that the distances are directly as the square roots of the number of candles; or in other words, that the intensities of the lights will be inversely as the squares of the distances. If gas lights be employed, having burners capable of consuming known quantities of gas in equal times, and the photometer be placed between them, so that the effect upon the air in each chamber shall be the same; it will be found that the quantities of gas consumed by each, will be exactly proportional to the squares of the distances of their respective flames from the ends of the photometer."

"This instrument seems well adapted for determining the relative quantities of light given out by the combustion of coal and oil gas. Place the instrument as before between the two burners, and ascertain the relative intensities of the two lights, by squaring their distances from the adjacent ends of the instrument; as-

certain the quantities of gas consumed by each of the burners in the same time; multiply these quantities by the squares of the respective distances, and the product will be the relative quantities of light, afforded by the gases. Let d be the distance of the coal gas light, and d' that of the oil gas light; and let q be the quantity of coal gas consumed in a given time, and q' the quantity of oil gas consumed in the same time, then the intensity of the coal gas will be to that of the oil gas $q\ d^2$ to $q'\ d'^2$."

The following paragraph we do not clearly understand.

"To find the ratio between the quantities of light given out by the sun, and that afforded by a common candle, place one end of the instrument opposite the sun, and bring the candle opposite the other end, till the fluid in the stem remain stationary at the original division, and the light given out by the candle, will evidently be to that given out by the sun, as the square of a few inches to the square of the number of inches contained in 95,000,000 miles, provided none of the sun's light had been absorbed in its passage through the atmosphere."

If this instrument really possess the qualities ascribed to it by its author, it is well calculated for many important investigations to which neither Mr. Leslie's photometer, nor any similar gimcracks, are at all applicable; but we are somewhat apprehensive that Mr. Ritchie has over-valued the accuracy of its indications, as well as the facility of its employment. If not, we hope soon to hear more of it.

vii. *Description of a Floating Collimator. By* Captain Henry Kater, F. R. S.

The apparatus described in this paper is intended to determine the situation of the line of collimation of a telescope attached to an astronomical circle, with respect to the zenith or horizon, in some one position of the instrument.—In other words, to determine the zero point of the divisions on the limb. This is at present usually performed by the use of the level or the plumb-line, or by the reflection of an object from the surface of a fluid. The author describes the defects and inconveniencies of each of these methods. Those of the plumb-line, when applied to small instruments, (to the improvement of which he describes his attention to have been particularly directed,) are referrible chiefly to want of sufficient delicacy. Those of the level are referrible to a variety of causes not under the command of the observer. While observations by reflection, the most perfect perhaps of any now practised, require a union of favourable circumstances rarely occurring. Add to these, when levels or plumb-lines are used, the necessity of reversing the instruments—and observing out of the meridian—and when observations are made by reflection that of deferring the corresponding

observation to the following night, which has proved so great an inconvenience at Greenwich, as to necessitate the erection of a second circle for the purpose of simultaneous observation.

The principles on which the floating Collimator is constructed are two—the first is the property of a telescope employed by Mr. Gauss, and subsequently by Mr. Bessel, in virtue of which the cross-wires of a telescope, adjusted to distinct vision on the stars, may be distinctly seen by another telescope, also so adjusted, at whatever distance the telescopes may be placed, provided their axes coincide,—the rays diverging from the cross-wires of either telescope, emerging parallel from its object-glass, and being therefore refracted by that of the other telescope to its sidereal focus, as if they came from an infinite distance. The author here translates an account by Professor Bessel of a method of using this principle to determine the horizontal or zenith point of a circle by the use of a level, employed to place the *collimating* or subsidiary telescope in a horizontal position—a method which, though characterized by him as the best mode of using a level that has been yet devised, is still liable to the objections urged against levels in general.

The other principle, which the author substitutes in the place of the level, is the invariability with respect to the plane of the horizon of a body of determinate figure and weight, floating on the surfaee of a fluid. In former inquiries he had satisfied himself practically that a body floating on mercury might be so contrived as to have always, when at rest, the same inclination to the horizon. He had thus a floating support to which he could attach a telescope—a support requiring no adjustment, offering the ready means of extreme accuracy, and precluding all fear of those errors which might arise from the use of a level.

The collimator, in its perfect state, consists of a piece of cast iron about 8 inches long, 4 wide, and from a quarter to half an inch thick, having two uprights in the form of Y's, in which the collimating telescope is firmly fastened. The support is then floated on mercury in a deal box, somewhat larger than the flat portion of the iron, and having its bottom just covered with mercury. The float is kept in its situation in the middle of the box, and prevented from moving horizontally by two smooth iron pins projecting from its sides, and moving freely in vertical polished grooves of metal let into the sides of the box. The whole of the telescope projects above the edges of the box, and a screen of black pasteboard, with an aperture equal to that of its object-glass, is fixed to the end of the box to keep off false light. The instrument was placed on a table attached to the wall of the observatory, and directed (by looking through its telescope) to the wires of a fine achromatic, furnished with a wire micrometer. The cross-wires of the collimator were then illuminated by a

small lantern placed behind its eye-glass, with oiled paper interposed.

The object of the author in this arrangement being to ascertain the limits of variability in the position assumed by the collimator, it was deranged purposely in a variety of ways, by removing and replacing the float, or carrying the whole instrument from its place, and every method he could think of used that could fairly introduce error. His preliminary trials were made with a wooden float, but this was soon laid aside after ascertaining that the greatest single error committed in using it did not exceed 2″.58 in the position of the horizontal point. Other floats were then tried, and it was found that the increase of their length, and browning their surfaces with nitric acid, produced material advantages. In 151 single results thus experimentally obtained, 28 only were found to give errors in the determination of the horizontal point exceeding 1″., and only two amounting to 2″.; but if the means of every successive five be taken, and the experiments with the wooden float rejected, the greatest error did not exceed 0″.4, and even here the influence of a constant source of error depending on the support of the micrometer employed was apparent.

The author then describes at length the mode of using the collimator, and of observing with it. The instrument hitherto described may be called the *horizontal* collimator; but he then proceeds to describe a *vertical collimator*, in which the telescope is fixed perpendicularly to the float, and placed immediately under the axis of the circle. By this arrangement the necessity of transporting it from one side of the observatory to the other is avoided, the reverse observation being made by merely turning the float half round in azimuth.

It is not necessary that the telescope of the collimator should have a tube, nor does the author appear to regard its length as of any importance, it being merely the *direction* of its axis which is the subject of examination, and the accuracy of this examination will depend on the length and power of the telescope of the circle to be collimated. The adjustment of the cross-wires in the exact sidereal focus of its object-glass is, however, a point of the highest importance.

The author next points out an important advantage which this instrument presents, *viz.*, that of enabling the observer, by varying the inclination of his float, to detect erroneous divisions of his circle, by bringing different parts of its arc into use.

After which he proceeds to describe an application of his floating collimator as a permanent verification of the verticality of a zenith tube; and considers that, by its use, the error, if any, in the zenith distance of a star, will be ultimately referred to inac-

curate bisection of the star, or imperfections in the micrometer screw.

viii. *Notice on the Iguanodon, a newly-discovered Fossil Reptile, from the Sandstone of Tilgate Forest, in Sussex. By* Gideon Mantell, F.L.S. *and* M.G.S., *Fellow of the College of Surgeons, &c. In a Letter to* Davies Gilbert, *Esq.*, M.P., V.P.R.S., *&c. &c. &c. Communicated by* D. Gilbert, *Esq.*

The sandstone of Tilgate Forest is a portion of that extensive series of arenaceous strata, which constitutes the *iron sand formation*, and in Sussex forms a chain of hills that stretches through the county in a W.N.W. direction, extending from Hastings to Horsham. In various parts of its course, but more particularly in the country around Tilgate and St. Leonard's Forests, the sandstone contains the remains of saurian animals, turtles, birds, fishes, shells, and vegetables. Of the former, three, if not four, species belonging to as many genera are known to occur, *viz.*, the crocodile, megalosaurus, plesiosaurus, and the iguanodon, the animal whose teeth form the subject of this communication. The existence of a gigantic species of crocodile in the waters which deposited the sandstone, is satisfactorily proved by the occurrence of numerous conical striated teeth, and of bones possessing the osteological characters peculiar to the animals of that genus; of the megalosaurus, by the presence of teeth and bones resembling those discovered by Professor Buckland in the Stonesfield slate; and of the plesiosaurus, by the vertebræ and teeth analogous to those of that animal.

The teeth of the crocodile, megalosaurus, and plesiosaurus, differ so materially from each other, and from those of the other lacertæ, as to be identified without difficulty; but in the summer of 1822, others were discovered in the same strata, which, although evidently referrible to some herbivorous reptile, possessed peculiar characters. Mr. Mantell, therefore, endeavoured to discover such connected portions of the skeleton as might determine the nature of their former owners, but in vain; but on comparing them with the teeth of certain recent lacertæ, he discovered their correspondence with those of a species of iguana, which are delineated in annexed plate.

Whether the animal to which the fossil teeth belonged should be considered as referable to existing genera, differing in its specific characters only, cannot at present be determined. "But," says Mr. M., "if any inference may be drawn from the nature of the fossils with which its remains are associated, we may conclude, that if amphibious, it was not of marine origin, but inhabited rivers or fresh-water lakes; in either case the term iguanodon, derived from the form of the teeth, (and which I

have adopted at the suggestion of the Rev. W. Conybeare) will not, it is presumed, be deemed objectionable."

IX. *An Experimental Inquiry into the Nature of the radiant heating Effects from terrestrial Sources. By* Baden Powell, M.A., F.R.S., *of Oriel College, Oxford.*

The general conclusions to be deduced from the experimental details of this paper are as follow:—

1st. That part of the heating effect of a luminous hot body, which is capable of being transmitted in the way of direct radiation through glass, affects bodies in proportions to their *darkness of colour*, without reference to the texture of their surfaces.

2d. That which is intercepted produces a greater effect in proportion to the *absorptive nature of texture* of the surface, without respect to colour. These two characteristics are those which distinguish simple radiant heat at all intensities.

Thus when a body is heated at lower temperatures, it gives off only radiant heat stopped entirely by the most transparent glass, and acting more on an absorptive white surface than on a smooth black one.

At higher temperatures the body still continues to give out radiant heat, possessing exactly the same characters.

But at a certain point it begins to give out light: precisely at this point it begins also to exercise another heating power distinct from the former; a power which is capable of passing directly through transparent screens, and which acts more on a smooth black surface than on an absorptive white one.

This last sort of heat, whatever its nature may be, is essentially different from simple radiant heat. It appears to agree very closely with what the French philosophers term "calorique lumineux," and is, according to Professor Leslie's theory, a conversion of light into heat. These views of the subject are certainly gratuitous assumptions. We have no right whatever to identify those two agents, or to suppose that, because a heating effect very closely accompanies the course of the rays of light, the light is therefore converted into heat; but the theories above alluded to, seem to regard the *whole* heating effect of a luminous body as of this latter character. In this particular, the present inquiry has led us to an essential distinction; and if the experiments are to be relied upon, this peculiar sort of heat constitutes only *a part* of the total effect. These results do not indeed present so simple a theory as that alluded to, but they apply very obviously to the explanation of many phenomena recorded by various experimenters.

The peculiar heat above spoken of, and which, for the sake of distinction and brevity, we may call "transmissible heat," is similar to that which acts in the solar rays, and which there con-

stitutes the total effect. It is this kind of heat which has been employed as a principle of photometry, on the assumption that it is precisely proportional to the intensity of light. Within certain limits this may be the case; but there are unquestionably circumstances under which the relation is very different; such, for example, as difference of colour in the light: and in general it cannot be assumed to hold good in light from different sources. To show this, there is a remarkable instance in incandescent metal, which produces but very faintly illuminating rays, yet its "transmissible heat" is very considerable. "I have repeatedly," says Mr. Powell, "tried the experiment with a small 'photometer,' having one bulb painted with Indian ink, and the other plain; the bulbs being in a vertical line; this instrument whether employed with or without its case, or a glass screen, always gave an effect of about 10° in 30" at eight inches distance from a ball of iron heated to the brightest point in a common fire."

In making these last experiments, the effect was always greater when the instrument was used without its case, or a glass screen. This was no doubt in part owing to the greater action of the simple heat now admitted to the instrument on the coated, than on the plain bulb; but it was also in part occasioned by the circumstance, that the stem going to the upper bulb passes in contact with the lower, and being a solid mass compared with the thin bulb, is slower in acquiring heat, and therefore cools it, thus increasing the apparent effect on the other.

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11. *Essai Géognostique sur le Gisement des Róches dans les deux Hemispheres, par* Alexandre de Humboldt. 1 vol. 8vo. F. G. Levrault, Paris, 1823.

It is known that Baron Alexander Humboldt is a great traveller, and it is believed that he is a great Astronomer, a great Botanist, a great Political Economist, and, to sum up all, a great Philosopher. It remained to be proved that he was also the great Geologist that he had been believed; and the proof is, the book before us.

The Title, implicating the whole sphere, (which is obviously produced by the addition together of two hemispheres,) would have led us to expect the history of all the rocks of our sphere, or globe, had we not been accustomed to the grandiloquism of our author; and we ought therefore to inform our readers, that the "gisements" in question are confined to a very small portion of Europe, and a much smaller one of America. The other two hemispheres, Asia and Africa, will probably appear in a future volume.

But this work embraces "pour ainsi dire, toute la geognosie positive;" a fact which we are very happy to hear, as the shelves of the Geological Society may now discard the lumber, German, French, and Italian, besides English, with which they are loaded, and replace it with this volume of quintessence.

Before proceeding further, however, we ought probably to apologize to Baron Humboldt, to our readers, and to what is commonly called The World, for daring to take liberties with a Great Man, or a Great Name rather. It is the courtesy of society to fall down and worship the idol which it has set up, and thus that Scotch Entity, called the Great Unknown, is permitted to write with impunity, even *St. Ronan's Well*. All that we can say, however, in our defence is, that whatever respect we may have for Trojan or Tyrian, we have more for truth; and that if Plato himself had written on the two hemispheres, we should have equally asked what he knew of them.

Why will not M. Humboldt tell a plain tale plainly? If he had useful geological information to give on an unknown country, he might have done it without all this *pretension*. His book is not a system of Geognosy, and it is not a description of the positions of the rocks in the two hemispheres. The Equator, the Andes, the Amazon, the Oroonoco, are sonorous words, but the world already knows, and is wearied of hearing, that he has travelled them; weary of his travels, weary of his latitudes, weary of his isothermal lines, weary of his geography of plants and animals, and weary of him. The very Faubourg St. Germain, the saloons, the coteries, are as weary of the Andes as are Cuvier, Arago, and Fourrier.

We must not, however, transgress the circle here drawn round us; yet, in this narrow book, we can find matter for caution and advice, which, in sincerity and friendship, whatever M. Humboldt may think, we offer to him. He has seen much that others have not; he has collected facts that others have not; he has travelled, and he might have instructed. But not content with such fame, and such praise, as he might have commanded, he has been resolved to be a philosopher and a Sublime; to Chateaubriandize on weeds, and Algebraize on stones, forgetting that he who has placed one foot on the pinnacle is thus much nearer to the bottom—" il n'y a qu'un pas."

There is a certain singleness and simplicity of apprehension necessary to writing clearly, and this is a quality, we grieve to say, which M Humboldt does not possess. If there be an idea, it is involved in such a mass of words that it is suffocated; if there be two, they become so entangled that their separate identity disappears. The Baron's writing appears to have sat for its portrait to M. Fuseli's lectures on painting. The purpose of language, says Swift, (among others,) is to conceal your

thoughts; but it is another important purpose, which he has overlooked, to be a substitute for them.

There are always two terms, a good and a bad one, for every human action; for every thing. Michael Angelo is grand, or extravagant and absurd. M. Humboldt professes to generalize; his views are grand, broad, poetical; or else they are fictitious, slovenly, or *dreamy*. The mob has voted "the ayes;" we are of the "contrary opinion." In philosophy, in science, natural or otherwise, we know of no generalization that is not a collation and abstract of facts. Without this, it is all guess and fancy; and the fancies are commonly abundantly dull, moreover. Isothermal curves may captivate the uninquiring herd, as do the crotchets of algebra, since they look profound, like Lord Burleigh; but, like Lord Burleigh, it is to say nothing. We should be much more grateful to M. Humboldt if he would particularize; we do not want pages of guessing; and if we are to read poetry, let it be *John Gilpin* or *Childe Harold:* nor "Goddess, write, about it and about it."

Can we now convince M. Humboldt that we are his friends and well-wishers: if we tell him of his faults, where else will he find sincerer ones? But we must proceed to the "*Gisement des Rôches*. And here also we must premise that we find it utterly impossible to translate the quotations which we shall be compelled to make. No language but that of the original would justify our criticisms; nor, while our translation of confusion into order and sense, would disarm us, are we very willing to undergo the toil of licking these cubs into a bear-like form.

Upwards of sixty pages are occupied in something that looks like an introduction, being a loose collection of facts, words, and opinions; and this is followed by an account of all the acknowledged rocks, from granite upwards. Such is the plan of the work, which is nearly as undigested as its materials are indigestible.

To give any thing like an analysis of the introductory matter, which seems intended as a sort of abstract of the science, is impossible. We are lost amid the vagueness of what might have been compressed into half the space, or less, while it ought to have been divided into paragraphs, or subjects. It is not thus that philosophy is to be written or taught; and if this be Geognosy, we shall remain content with simple Geology. If there is a want of precision, of *definiteness*, in the manner, there is not less in the matter; while, with a mixture of pedantry and modernism, there is that antiquated phraseology, derived from Freyberg, which shows that the author has not reached up to the level of our present knowledge, but is merely ingrafting a few facts from a better source, on the cant and dogmas of his school. The want of purpose, the vagueness of ideas, the parade of

science, which are so characteristic of this writer, will perhaps be rendered as visible by the following passage as by any other; and it is really selected without any invidious motives, and as an example of his style and turn of thinking.

"Il ne faut pas confondre, et j'ai souvent insisté sur ce point, dans cet article, des roches passant insensiblement à celles qui sont en contact immédiat avec elles, par exemple, des micaschistes qui *oscillent* entre le gneis et le thonschiefer, avec des roches qui alternent les unes avec les autres, et qui conservent tous leurs caractères distinctifs de composition et de structure. M. d'Aubuisson a fait voir, il y a long-temps, combien l'analyse chimique rapproche le thonschiefer du mica. (*Journal de Physique*, T. 68, pag. 128; *Traité de Géognosie*, T. ii. pag. 97.) Le premier, il est vrai, n'a pas l'éclat métallique du micaschiste; il renferme un peu moins de potasse et plus de carbone; la silice ne s'y réunit pas en nœuds ou lames minces de quarz comme dans le micaschiste: mais on ne peut douter que des feuillets de mica ne constituent la base principale du thonschiefer. Ces feuillets sont tellement soudés ensemble, que l'œil ne peut les distinguer dans le tissu. C'est peut-être cette affinité même qui empêche l'alternance des thonschiefer et des micaschistes: car dans ces alternances la nature semble favoriser l'association de roches hétérogènes; ou, pour me servir d'une expression figurée, elle se plaît dans les associations dont les roches alternantes offrent un grand contraste de cristallisation, de mélange et de couleur. Au Mexique j'ai vu des grünstein vert-noirâtre alterner des milliers de fois avec des syénites blanc-rougeâtre et qui abondent plus en quarz qu'en feldspath: il y a dans ce grünstein des filons de syénite, et dans la syénite des filons de grünstein; mais aucune des deux roches ne passe à l'autre. (*Essai Politique sur la Nouvelle Espagne*, T. ii. p. 523.) Elles offrent sur la limite de leur contact mutuel des différences aussi tranchées que les porphyres qui alternent avec les grauwackes ou avec les syénites, que les calcaires noirs qui alternent avec les thonschiefer de transition, et tant d'autres roches de composition et d'aspect entièrement hétérogènes. Il y a plus encore: lorsque dans des terrains primitifs des roches plus rapprochées par la nature de leur composition que par leur structure ou par le mode de leur agrégation, par exemple, les granites et les gneis, ou les gneis et les micaschistes, alternent, ces roches ne montrent guère cette même tendance de passer les unes aux autres qu'elles présentent isolément dans des formations non complexes. Nous avons déjà fait observer plus haut que souvent une couche β, devenant plus fréquente dans la roche α, annonce au géognoste voyageur qu'à la formation simple α va succéder une formation complexe dans laquelle α et β alternent. Plus tard il arrive que β prend un plus grand développement; que α n'est plus une roche alternante, mais une simple couche subordonnée à β, et que cette

roche β se montre seule jusqu'à ce que par la fréquente apparition de couches γ elle prélude à une formation complexe de β alternant avec γ. On peut substituer à ces signes les mots de granite, gneis et micaschiste ; ceux de porphyre, grauwacke et syénite ; de gypse, marne et calcaire fétide (stinkstein.) Le langage *pasigraphique* a l'avantage de généraliser les problèmes ; il est plus conforme aux besoins de la *philosophie géognostique*, dont j'essaie de donner ici les premiers élémens, en tant qu'ils ont rapport à l'étude de la superposition des roches. Or, si souvent entre des formations simples et très-rapprochées dans l'ordre de leur ancienneté relative, entre les formations α, β, γ, se trouvent placées des formations complexes, $\alpha\beta$ et $\beta\gamma$ (c'est-à-dire α alternant avec β, et β alternant avec γ) ; on observe aussi, quoique moins fréquemment, qu'une des formations (par exemple, α) prend un accroissement si extraordinaire qu'elle enveloppe la formation β, et que β, au lieu de se montrer comme une roche indépendante, placée entre α et γ. n'est plus qu'une couche dans α. C'est ainsi que dans la Silésie inférieure le grès rouge renferme la formation du zechstein ; car le calcaire de Kunzendorf, rempli d'empreintes de poissons, et analogue à la marne bitumineuse et abondante en poissons de Thuringe, est entièrement enveloppé dans le grès houiller. (Buch. *Beob.* t. i. p. 104, 157 ; *Id.*, *Reise nach Norwegen*, t. i. p. 158 ; Raumer, *Gebirge von Nieder-Schlesien*, p. 79.) M. Beudant (*Voy. Min.*, t. iii. p. 183) a observé un phénomène semblable en Hongrie. Dans d'autres régions, par exemple, en Suisse et à l'extrémité méridionale de la Saxe, le grès rouge disparoît entièrement, parce qu'il est remplacé et pour ainsi dire vaincu par un prodigieux développement du grauwacke ou du calcaire alpin. (Freiesleben, *Kupfersch.*, b. iv. 109.) Ces effets de l'alternance et du développement inégal des roches sont d'autant plus dignes d'attention, que leur étude peut jeter du jour sur quelques déviations apparentes d'un type de superposition généralement reconnu, et qu'elle peut servir à ramener à un type commun des séries de gisement observées dans des pays très-éloignés."

We might make many commentaries on this extract, but shall content ourselves with few. We do not see any necessity for "insisting" on our not confounding transitions of rocks into each other with mere alternations ; and cannot perceive the vast value of the caution, nor the importance of the enunciation, as if it were the author's own discovery. As to M. d'Aubuisson, with his mica and thonschiefer, he is worthy of Humboldt. We may ask either, or both, what on earth the chemical analysis of a mixed rock proves, and what important consequences follow from knowing that mica and clay-slate contain the same earths. The analysis of a hundred sandstones would correspond with that of as many granites ; hornblende contains the same earths as clay-slate, and in the same proportion ; so does shale, so does clay itself, so does

basalt, so does greenstone, and so does pitchstone. We may ask, with Voltaire's "Nonchalant," What then? As to our "not doubting that scales of mica form the base of clay-slate," we certainly not only doubt, but deny it. The one is a crystal, or a crystallized mineral, the other not; and if we were to seek for an identity "not to be doubted," it would rather be between clay and clay-slate.

That nature "favours the association of heterogeneous rocks," is one of those frivolous and false generalizations which characterize the whole of this author's writings. It is not the fact, and if it were, what then? If there be great spaces or depths of one rock without intermixture, why then there are; and if there are not, why then nature must "please herself" with differences. These are words; and thus it is with this author, as if a different wordage was philosophy and a discovery, a generalization and *une belle idée.*

There is another of these general laws, equally unmeaning, and, to say truth, not exactly intelligible, at least in English, whatever it may be in French or German geognosy, or in the geognosy of New Spain. It is that, "when in the primitive class, rocks more akin in composition than in structure or aggregation, such as granite and gneiss, or gneiss and mica-slate, alternate, "Ces roches ne montrent guère cette même tendance de passer les unes aux autres qu'elles présentent isolément dans des formations non complexes." It is not by discovering such laws as this that geognosy, or any gnosy will ever become a science.

Here also we stumble on the threshold of the author's new geognostic algebra; and unquestionably, there is much profundity in a pasigraphy of alpha, beta, gamma, delta; but as we find that this new lithological algorithm is lying in wait for us again in the Appendix, we shall for the present pass it over, as well as the remainder of a paragraph which, thus far, we owed to our readers.

There are many ways of writing a new book, or a new paragraph, or of seeming to have discovered a new idea, or to have made a discovery; and there is none much more effective than that of translating a plain, vulgar, intelligible, well known fact or proposition, into long-winded, altisonant, and obscure phraseology. For example. It does not follow that, because any given limestone, such as that which lies beneath the red marl, contains pectines or terebratulæ in England or Greenland, it must also contain them in Peru or Napaul, and it is not true that because an echinus belongs to chalk, it must not also exist in slate. Nothing can be plainer; the truth is, that it is a great deal too plain. Now mark how it is improved by dress, how it is adornized, till we are as puzzled to recognise it as we should be to know Mr. Waithman, when tricked up in all the robings and state of a Lord Mayor.

"Dans le cas de la conformité de gisement, il peut y avoir identité de masse (c'est-à-dire de composition minéralogique) et diversité de fossiles, ou diversité de masse et identité de fossiles. Les roches β et β' placées à de grandes distances horizontales entre deux formations identiques α et γ, ou appartiennent à une même formation, ou sont des formations parallèles. Dans le premier cas, leur composition minérale est semblable; mais, à cause de la distance des lieux et des effets climatériques, les débris organiques qu'elles renferment, peuvent différer considérablement. Dans le second cas, la composition minéralogique est différente, mais les débris organiques peuvent être analogues. Je pense que les mots, *formations identiques*, *formations parallèles*, indiquent la conformité ou non-conformité de composition minéralogique, mais qu'ils ne font rien préjuger sur l'identité des fossiles. S'il est assez probable que des dépôts β et β', placés à de grandes distances horizontales entre les mêmes roches α et γ, sont formés *à la même époque*, parce qu'ils renferment les mêmes fossiles et une masse analogue, il n'est pas également probable que les *époques de formation* sont très-éloignées les unes des autres, lorsque les fossiles sont distincts. On peut concevoir que sous une même zone, dans un pays de peu d'étendue, des générations d'animaux se sont succédé, et ont caractérisé, comme par des types particuliers, les *époques* des formations; mais, à de grands éloignemens horizontaux, des êtres de formes très-diverses peuvent, sous differens climats, avoir occupé simultanément la surface du globe ou le bassin des mers. Il y a plus encore: le gisement de β entre α et γ prouve que la formation de β est antérieure à celle de γ, postérieure à celle de α; mais rien ne nous donne la mesure absolue de l'intervalle entre les *époques-limites*, et différens dépôts (isolés) de β peuvent ne pas être simultanés."

The man who, like Mr. Hazlitt, proposes to cover a certain surface of paper with a certain number of words, would be very silly if he were to trouble himself about ideas, propositions, or arguments. The public expects nothing but a certain jack-o'-lantern flitting and dancing, which amuses the eye without encumbering the thinking powers; and it is satisfied. It reads, because it is necessary or convenient to read; and the repose of its faculties remains undisturbed. And this is very charming reading too; as they will vouch, who have scanned, with never-exhausted delight, the exquisite verses of a "Person of Quality," which the prophetic mind of Pope sent forth, a model to succeeding ages. But he who, like Eudoxus or Euclid, deigns to assert some fact, or to give reasons why a fact should be believed, or to do both the one and the other, has generally been desired to tell the nature of his proposition and to assign his reasons. The very tailor himself, is, indeed, unreasonable enough to expect something of this kind, when he is to make a coat, and when

it is to be, or not to be, made with a Brummel collar, or a Petersham pocket. In all the sciences, from piquet to political economy, as Hoyle and Adam Smith have shewn, and even in geology or geognosy, those who either understand, or desire that others should understand, usually state propositions and subjoin reasons; and, most commonly, when a man writes a paragraph, it is a paragraph, and nothing else. It contains a head, a body, and a tail, much in the manner of a horse, or of one of Cicero's orations; and does not run out into protuberances, or comprise various heads and tails, or, beginning with the history of a cock, terminate with that of a bull.

The figure called rigmarole has not generally been held in very particular esteem in the mathematical sciences. It is lucky, however, that it is approved of by the geognosts; and really there is such an affinity, and a harmony, between the two categories of geognosy and rigmarole, that we begin to think ourselves in error in doubting the utility and adaptation of the manner before us to the matter. We were about to have said, "De grace, Monsieur le Baron, humanisez votre discours, et parlez pour être entendu." But to what purpose, when there is nothing to be understood?

We have heard of a man running away with an idea, and we have also heard of an idea running away with a man. We have no great objection to either. But when a man is "run away with" by half a dozen at a time, his sentences and paragraphs are apt to become as disjointed as he himself would be, had he been run away with by as many wild horses. There is a vulgar maxim, called sticking to the point, which has been sometimes recommended, and which might be advantageously applied somewhat more widely. As, for example—

"Il résulte de ces considérations générales sur les caractères zoologiques et sur l'étude des corps fossiles, que, malgré les beaux et anciens travaux de Camper, de Blumenbach et de Sömmering, l'exacte détermination spécifique des espèces, et l'examen de leurs rapports avec des couches très-récentes et voisines de la craie, ne datent que de vingt-cinq ans. Je pense que cette étude des corps fossiles, appliquée à toutes les autres couches secondaires et intermédiaires par des géognostes qui consultent en même temps le gisement et la composition minérale des roches, loin de renverser tout le système des formations déjà établies, servira plutôt à étayer ce système, à le perfectionner, à en compléter le vaste tableau. On peut envisager sans doute la science géognostique des formations sous des points de vue très-différens, selon que l'on s'attache de préférence à la superposition des masses minérales, à leur composition (c'est-à-dire, à leur analyse chimique et mécanique), ou aux fossiles qui se trouvent renfermés dans plu-

sieurs de ces masses; cependant la science géognostique est une. Les dénominations, *géognosie de gisement* ou *de superposition*, *géognosie oryctognostique* (analysant le tissu des masses,) *géognosie des fossiles*, désignent, je ne dirai pas, des embranchemens d'une même science, mais diverses classes de rapports que l'on tâche d'isoler pour les étudier plus particulièrement. Cette unité de la science, et le vaste champ qu'elle embrasse, avoient été très-bien reconnus par Werner, le créateur de la géognosie positive. Quoiqu'il ne possédât pas les moyens nécessaires pour se livrer à une détermination rigoureuse des espèces fossiles, il n'a cessé, dans ses cours, de fixer l'attention de ses élèves sur les rapports qui existent entre certains fossiles et les formations de différens âges. J'ai été témoin de la vive satisfaction qu'il éprouva, lorsqu'en 1792 M. de Schlottheim, géognoste des plus distingués de l'école de Freiberg, commença à faire de ces rapports l'objet principal de ses études. La géognosie positive s'enrichit de toutes les découvertes qui ont été faites sur la constitution minérale du globe; elle fournit à une autre science, improprement appelée *théorie de la terre*, et qui embrasse l'histoire première des catastrophes de notre planète, les matériaux les plus précieux. Elle réfléchit plus de lumières sur cette science qu'elle n'en reçoit d'elle à son tour; et, sans révoquer en doute l'ancienne fluidité ou le ramollissement de toutes les couches pierreuses (phénomène qui se manifeste par les corps fossiles, par l'aspect cristallin des masses, par les cailloux roulés ou les fragmens empâtés dans les roches de transition et les roches secondaires), la géognosie positive ne prononce point sur la nature des ces liquides dans lesquels, dit-on, les dépôts se sont formés, sur ces *eaux de granite, de porphyre et de gypse*, que la géologie hypothétique fait arriver, marée par marée, sur un même point du globe."

For one paragraph, this is somewhat lengthy, as Jonathan might say. What the proposition may be, is another question. Is it fossil remains, or a method how, to "envisager" geognosy, or to "envisager" the merits of Werner, or about a theory of the earth which is not a theory, or to prove that, though Werner is the greatest geognost that ever lived, he had no business to make "ces eaux de granite de porphyre, et de gypse," arrive "marée par marée sur un même point du globe." When a man sits down to write a paragraph, he would find it convenient to ascertain what he means to say. He should take a long breath also, lest he run himself out of wind; an accident which we have observed to happen, in conversation, to those who deal in these same interminable paragraphs.

As to what may be the main matter, or rather one of the main matters of the aforesaid paragraph, we really have some doubts respecting the creative powers of Werner in positive geognosy.

The positive quality we grant; as this geognosy never betrayed any want of confidence and assertion. The remainder might be mooted, if we were much inclined that way. We remember the late Mr. Tennant, and we remember his pronouncing in one of his lectures respecting this creator of positive Geognosy,—" a stupid German blockhead"—: "aside," however, as is sometimes the usage of the stage. Really, Mr. Tennant, these were hard words; but Mr. Tennant had not scaled Cotopaxi or Popocatepetl. Dans le pays des aveugles les borgnes sont Rois.

There is a vast virtue in hard words, just as there is in the *x*'s and *y*'s of Algebra. Baron Humboldt is not unacquainted with the value of " Loxodromisme," however we may be. The office near Soho-square for plundering, alternately, servants and masters, would lose half its attractions but for its Therapolegia inscription. The guinea paid for supplying it, was well bestowed. Loxodromism, or Loxodromy. What a mass of knowledge is contained in this little word. Elevation, parallel position, these are household terms, and carry no weight. The clown does not admire the preacher whom he understands: " he is no Latiner." When a man can express himself in the ordinary language which he has picked up from his nurse, we have generally a suspicion that he knows what he means. Solomon seemed to be of the same opinion when he complained of the man who " darkened counsel by words without knowledge."

This Greek coinage is a false money, which cheats us with a semblance for solid gold. We wish the French nation would burn its Lexicons, since it has not yet learnt to make any other use of them. Great evils arise from petty causes, now and then. Had Brongniart not been the lucky, (unlucky for us), possessor of a Greek Lexicon, we should have been spared Phyllade, and Dolerite, and Trachyte, and Psammite, and Euphotide, and Diabase, and heaven knows what more; we should have been spared the arrangement, and the system, and the whole, and we might now have understood what his nation is writing about, if indeed they understand it themselves.

But the Greek is not the sole criminal, for the German does as well. Geognosy, or aught else, there is nothing so easy as phrases, phraseology, conventional language; cant, we were very nearly saying. All the Gnosies have their phrases and their cant; and words go for sense. They save the trouble of thinking, which is a vast convenience; and of understanding, which is more convenient still. Explain, explain, as they say in the House of Commons. Read Swift, and learn to write your own language: Read Blaise Pascal, Mons. le Baron.

But we must return to the Loxodromy, the *oblique course* of

the strata; and of Mr. Humboldt's ideas. And now we shall see the reasons which induced the GREAT TRAVELLER to travel to Pichinca and Portocabello.

J'ai éte, dès l'année 1792, très-attentif à ce parallélisme ou plutôt à ce *loxodromisme* des couches. Habitant des montagnes de roches stratifiées où ce phénomène est très-constant, examinant la direction et l'inclinaison des couches primitives et de transition, depuis la côte de Gênes, à travers la chaîne de la Bochetta, les plaines de la Lombardie, les Alpes du Saint-Gothard, le plateau de la Souabe, les montagnes de Bareuth et les plaines de l'Allemagne septentrionale, j'avois été frappé, sinon de la constance, du moins de l'extrême fréquence des directions *hor.* 3—4 de la boussole de Freiberg (du sud-ouest au nord-est). Cette recherche, que je croyois devoir conduire les physiciens à la découverte d'une grande loi de la nature, avoit alors tant d'attraits pour moi, qu'elle est devenue un des motifs les plus puissans de mon voyage à l'équateur. Lorsque j'arrivai sur les côtes de Venezuela, et que je parcourus la haute chaîne du littoral et les montagnes de granite-gneis qui se prolongent du Bas-Orénoque au bassin du Rio Negro et de l'Amazone, je reconnus de nouveau, dans la direction des couches, la parallélisme le plus surprenant. Cette direction étoit encore *hor.* 3—4 (ou N. 45° E.), peut-être parce que la chaîne du littoral de Venezuela ne s'éloigne pas considérablement de l'angle que fait avec le méridien la chaîne centrale de l'Europe. J'ai énoncé les premiers résultats que m'offroient les roches primitives et de transition de l'Amérique méridionale, dans un mémoire publié par M. de Lamétherie, dans son *Journal de Physique*, T. 54, p. 46. J'y ai mêlé (comme cela arrive souvent aux voyageurs, lorsqu'ils publient le résultat de leurs travaux pendant le cours même du voyage), à des observations trés-précises sur la grande uniformité dans la direction des couches (à l'isthme d'Araya, à la Silla de Caracas, au Cambury près Portocabello, sur les rives du Cassiquiare: voyez ma *Relat. hist.*, T. I, p. 393, 542, 564, 578, T. II, p. 81, 99, 125, 141), des aperçus généraux que j'ai regardés depuis comme vagues et moins exacts. Quatre années de courses dans les Cordillères ont rectifié mes idées sur un phénomène qui est beaucoup plus important qu'on ne l'avoit cru autrefois; et, de retour en Europe, je me suis empressé de consigner le résultat général de mes observations dans la *Géographie des plantes*, p. 116, et dans l'*Essai politique sur la Nouvelle-Espagne*, *T. II*, p. 520. L'indication de ce résultat étoit sans doute restée inconnue au savant auteur du *Critical examination of Geology* (p. 276), lorsqu'il a combattu les assertions publiées pendant mon absence, en 1799, par M. de Lamétherie."

Now, does Mr. Humboldt really conceive and assert that he

was the discoverer of the continuous and orderly elevations of strata, that this was an object to run away to the Equator about; and need he tell us, for the twentieth time, that he is *The Traveller* of New Spain, that he has published a Geographie des Plantes, an Essai politique sur la Nouvelle Espagne, a Relation Historique, and a book of personal narrative? As to the "importance" of the fact, he surely does not imagine that it was pointed out by him; and as to his ideas having been rectified by four years of running about the Cordilleras, we only grieve that he did not continue his "courses" for forty, that they might have undergone a general rectification.

We have no dislike to M. Humboldt. On the contrary, we esteem him as an industrious and amiable man, ambitious, busy, good-humoured, and, really, qualified, in many very important matters, for a general traveller. But his ambition has been of too vaulting a nature, and he has overleaped the point of success and security. He would write of every thing; and, on every thing, he has been vague, inaccurate, wordy, and wearisome. He would be the name to fill all Europe with its sound; and he has, for this purpose, filled it with his books. He has blown the trumpet for himself, and he has found friends to blow it for him. There is no base for all this building. It is too much the custom of the day to work up to some niche in the Temple of Fame, by contraband procedure, and it is not for M. Humboldt's sake alone, that we make these remarks.

Public Justice demands that all should have equal justice. Of fame itself, there never can be but a certain portion in the world; for, where all are rich, no one is wealthy. If it is to be monopolized, let it be allotted where it is merited; for, when it is allotted otherwise, the meritorious must be robbed of their share and their rights. The Journalist and Reviewer is the guardian of public justice in Literature and Science; and it is his duty, if often neglected, to see that praise is duly and justly allotted, that he may thus protect the feeble, or the neglected, whom the public will not protect, and who cannot protect themselves. That public will not listen to him, or about him, who is not A Humboldt (as the phrase is), or a something else; but it never stays to inquire what A Humboldt is, or what Humboldt has really done. For the sake of the injured and oppressed, for the sake of civil justice, we must strip off all that is fallacious or borrowed; we might do it for the purpose of criminal justice, as is the fashion with modern criticism, that we might punish him who deserves punishment. But this is not our object; and, from this intention, we desire that the subject of our remarks will exempt us.

We are not criticising the total works of this author, but we may say that he who has not simply been ambitious to shine alike as a general traveller, as an astronomer, a botanist, a geo-

logist, a politician, and much more, but who has laboured, directly and indirectly, to make the world believe all this, and who has contrived to succeed, ought to have been better informed, more accurate, and more dependent on his own powers and resources. Had he been all this indeed, we should have heard far less of him, for he would have been more modest, and modesty is not now the purchase-money of fame. There may be an excuse for bad writing, and confusion of thought and repetition, because an author cannot go beyond his powers. But there is little excuse for the perpetual ambition of being every thing; and there is none for inaccuracy, where accuracy is asserted, and where it is easily attained, nor for claims to originality founded on the labours of others. He at least who carries quadrants and wearies us with angles and immersions, is bound to be correct in longitudes, and most certainly in latitudes; and he who publishes the statistics of New Spain, would have judged more wisely had he informed us that they were a collection of public documents, and not the proper result of his own labour and observation.

But we must pass on to the work before us, and to the geology, or geognosy, of M. Humboldt. What we have extracted, will serve to shew the laxity and vagueness of this author's ideas, no less than of his writings. And it is not for the poor purpose of criticising him alone, that we have examined his work; but because, through him, we hope to induce other writers on the same subject, to introduce more precision and purpose into their geognostical writings. We are already encumbered with a mass of useless and unintelligible matter, fit only for bonfires; and if we are to go on thus, a library of geology will soon rival the libraries of physicians. This is an age in which every man can write something, and in which every man thinks it necessary to write a book on something. Geognosy has the especial merit of being an easy subject, "heaven bless the mark;" since by means of Hungary, Saxony, flötz, transition, and dolerite, with a little aid from loxodromism, hyenas, potamoid, acephalous, types, and a few more magic words, volumes are made, as volumes have been made before. Some mercy is due to the unhappy student, at least; and he who has thus seen the "high authority" of Humboldt doubted, may perhaps learn to doubt of other authorities, and perhaps also learn to fear lest the same measure may not be meted to himself, by future reviewers, less humane than ourselves.

Those who please to consider M. Humboldt as very great in any other science, must enjoy their belief; as that is their affair, not ours. But it is our affair, to say that we do not consider him a great geognost, practically, and that we do consider him a very useless writer, not to say more. His book is meant, apparently,

as it is declared, to be a system of geology, or geognosy; or a statement of all the facts of the science that have been ascertained; or a compendium, or detail, of "la geognosie positive." Assuredly, it is not that, in any sense. Of the introductory view, from which we have thus extracted, we can say nothing good or commendable. We have not selected passages to condemn, because they were faulty paragraphs and, by so doing, attempted to misrepresent the whole. There is no plan nor purpose, and no information, in this long preface, which, in the hands of science and knowledge, might have easily contained what it pretends to do; a general view of the state of geognosy, and of its principal facts and relations. It is a sort of *talk*, very much like the living conversation of the man himself; and we rise from the perusal without well knowing what we have been reading.

We have now, perhaps, said more than enough respecting this preface, yet we cannot part from it without recurring to a subject on which we touched before, and to a passage where the author becomes the critic and *laudator* of the Professor of Freyberg, of whose reign we really think it quite time that we should be weary. It is better to quote the passage, than to extract from it.

'Werner, en créant la science géognostique, a reconnu, avec une perspicacité digne d'admiration, tous les rapports sous lesquels il faut envisager l'indépendance des formations primitives, de transition et secondaires. Il a indiqué ce qu'il falloit observer, ce qu'il importoit de savoir: il a préparé, pressenti, pour ainsi dire, une partie des découvertes dont la géognosie s'est enrichie après lui, dans des pays qu'il n'a pu visiter. Comme les formations ne suivent pas les variations de latitude et de climats, et que des phénomènes; observés peut-être pour la première fois dans l'Himalaya ou dans les Andes, se retrouvent, et souvent avec l'association de circonstances que l'on croiroit entièrement accidentelles, en Allemagne, en Ecosse ou dans les Pyrénées; une très-petite portion du globe, un terrain de quelques lieues carrées dans lequel la nature a réuni beaucoup de formations, peut (comme un vrai *microcosme* des philosophes anciens) faire naître, dans l'esprit d'un excellent observateur, des idées très-précises sur les vérités fondamentales de la géognosie. En effet, la plupart des premiers aperçus de Werner, même ceux que cet homme illustre s'étoit formés avant l'année 1790, étoient d'une justesse qui nous frappe encore aujourd'hui. Les savans de tous les pays, même ceux qui ne montrent aucune prédilection pour l'école de Freiberg, les ont conservés comme bases des classifications géognostiques. Cependant, ce que l'on savoit en 1790 des terrains primitifs, de transition et secondaires, se fondoit presque entièrement sur la Thuringe, sur les montagnes métallifères de la Saxe et sur celles du Harz, sur une étendue de pays qui n'a pas

75 lieues de longueur. Les mémorables travaux de Dolomieu, les descriptions des Alpes de Saussure, furent consultés; mais ils ne purent exercer une grande influence sur les travaux de Werner."

We suppress the remainder of this long paragraph, as less important. What we have quoted is sufficient to shew that the critic is worthy of the subject, and each of the other. It will also tend to justify our own criticisms; for it is perfectly impossible, that any man really acquainted with geognosy, with the facts known, the history of the science, or its present state, could have formed such a judgment, or written such a paragraph.

Werner did not create "la science geognostique." He did little; and of that little, nearly all has proved to be wrong. There is not one of his general laws that has not been found utterly false; and if he ever became possessed of any facts, he proved that he was incapable of reasoning from them. The whole mass, nearly, of false induction and bad reasoning to be found in this science, is to be traced, directly or indirectly, to Werner and to Freyberg. It was, perhaps, a minor crime, that he taught what was untrue or useless, when he was the efficient cause of the ignorance and bad reasoning of a whole army of followers and admirers; and is still the night-mare of the science If we have begun to shake off his paralyzing influence in England, it is far otherwise in France and Germany; and we, as far as we can, will not allow M. Humboldt to go on blowing the trumpet, because he himself has no other ideas of geognosy than those which he derived at and from Freyberg. We have as little animosity against Werner, or his ghost, as we have against M. Humboldt, personally, or impersonally; but we will maintain, that to perpetuate the praises of him whose dogmas are the impediment of a rising science, whose assumed infallibility is the stumbling-block of students, is a crime that demands and deserves reprobation, because it is adding to the weight and immobility of the great obstacle to our progress in that science.

It would require a whole essay like this, instead of a paragraph or two, to examine the details of Werner's demerits in geognosy, and to prove him, in category, wrong. We cannot undertake to do it now, and moreover, it is a disgusting and a dull office. but what right has M. Humboldt to say, that the observations of Dolomieu "could not exercise any great influence over the labours of Werner?" If he meant it in irony, it would be true enough; since that dull and obstinate man shewed, through his whole life, that he was as incapable of making use of any one fact, and as unamenable to reasoning, as he was ignorant of all the necessary knowledge which was indispensable to the office he had undertaken. Wrapt up in his own unintelligible and impossible theories, he was satisfied with reigning a demigod or a pope

among the gaping herd that surrounded him, swallowing all his dogmas and doctrines, and repaying him with adulation and with volumes of his own "crambe recocta." Dolomieu might have taught him, if he had possessed sense enough to listen, that obsidian and pumice were not formed by water; nor volcanoes ignited by coal; in his own day, he might have learnt something of real geognosy from others than Dolomieu, if he had possessed talents or modesty enough to have become a pupil to those whom he professed to teach.

M. Humboldt also asserts, in a part of the paragraph which we have not quoted, that his great teacher exercised a surprising perspicacity in eliciting the truth from the confused narratives of travellers. Nothing so easy; and his pupils have also profited surprisingly by his example and his perspicacity. Nor any thing so easy as to support his system, or any system, by the same perspicacity. It was only to translate the language of any other geognost into his own, to know what the observer saw better than he knew himself, and the work was done. It is a fashion that is just as efficacious now. But we must take our leave of Werner, to return to M. Humboldt himself.

And we return to his sixty-seventh page, to his positive geognosy. This properly constitutes the book, and it is just such a collection of naked facts about rocks, as Kirwan wrote, or might have written, in his equally "admirable" work. If this comprises the whole of Positive Geognosy, we are very unfit reviewers of M. Humboldt's book, since we entertain somewhat different views about what is Positive Geognosy. In all the sciences, there are things, objects, and there are relations, actions, causes, analogies; categories, in short, of different kinds, which have generally been held essential parts of what is called a science. Perhaps, however, we have misapprehended the meaning of the term geognosy: but we do presume, that it implies somewhat more than what we find here, and that the adjective term, positive, does not form our author's justification for what he has given us.

It is indifferent where we take an example of this geognosy. We should have taken the first, which comprises the history of granite, but it is too long for extraction, and is beyond abridgment. If any of our geological readers choose to read it, we will permit them to judge for themselves, and shall not be very uneasy at the prospect of the judgment which they will pronounce between ourselves and the author, as to the general issue. They may select any other part, or read the whole; and if they can extricate any knowledge out of the mass of confusion, why then we shall envy them. Order, or system, there is none. We are dazzled with references to authors, and references to the new world, and hard names; and when we have laboured

through ten pages of porphyry or thonschiefer, it is all to be done over again; and when it is done, we ask ourselves what it is all about.

In truth, we did expect to have selected some single passage as an example; but, as far as the 240th page at least, we have turned backwards and forwards in vain, and cannot extract one sole paragraph which comprises an entire within itself. The author seems always breathless with matter, and the matter is itself breath, wind. A single fact, well detailed and simply stated, and, if the author ever did reason, reasoned on, would have been worth whole pages of Conradswalde, Prausnitz, Mondragon, Goldlauter, Schwartz, Kiffhauser, Caxamarca, and the ten thousand other names which dazzle our eyes at every sentence, and transport us, in an instant, from Saxony to Chili, as if we had been sitting on Prince Houssain's carpet.

We really cannot see the purpose of all this geography and authority, unless it be to show that M. Humboldt has got a map of the world at his elbow, and is a great traveller. Cannot he see that this is nothing more than an affair of Habitats, and that when once it is ascertained that the general relations of rocks are analogous or similar throughout the globe, it is unnecessary to torment us with every spot on his two hemispheres. The young botanist fancies he has performed a vast feat in science, when he has published, in some journal, a list of the plants in the king's park at Edinburgh, or in Norfolk. And it is a very fit occupation for a philosopher in the "Lovely Science." But that granite grows in the Fichtelgebirge, and at Tehuilotepetec, and that it consists, in both, of mica, felspar, and quartz, or of more mica and less felspar, is not now very marvellous information; and it is still less necesary that we should be told of every "gisement" in M. Humboldt's hemispheres, and of all and every variety and variation of every thing that he has seen, or not seen. Were any general laws deduced, we could understand a purpose in all this; but, as it is, it is egregious trifling, and trifling which pretends to knowledge. Foxes grow in Leicestershire and in Greenland, so they do in India, and, for ought we care, they may grow upon Popocatepetl; but in what way does the science of zoology profit by being told that their tails are an inch longer in Mexico than Bengal, or that they are less odoriferous in Norfolk than at Santa Fé da Bogotá.

The article Porphyry, positively dances before our eyes; and, what with transition, and dolerite, and trachyte, and all else of this never-ending subject, we are utterly unable to conjecture what he means to prove, or what he means. Ten sentences might possibly have told his meaning: they would have told ours; but he is not a man of ten sentences, and if he ever meant any thing e has lost sight of it, suffocated under the rubbish of his talk.

But we have arrived at the 360th page, and still we have been

unable to extract one quotation by which we might convey an idea of that talk. Our readers must really oblige us by undertaking a task to which we find ourselves unequal. It is a truth, and to us a sad one, that we really have read the book twice through, so that we cannot be accused of breach of duty; and it is not less true and sad, that it was less intelligible the second time than the first. Passages, facts, particulars of all kinds, we might criticise beyond endurance, but for what end? If, in our general remarks, our readers should esteem us severe, we will say, read, understand, explain; and we may say too, as Johnson said to Garrick, "If I have told the truth, Davy, why dost thou snub me?"

But there are more last words, and they will require a few last words from us.

We have already alluded to an air of *pretension* which pervades our author's writings; and, among other matters, to his algebra, forming one, but by no means all, of the modes in which it is displayed. We mean it for all whom it may suit, and not for him alone, when we say that this use of symbols and equations when the proposition can be given in common language, is either a piece of miserable affectation, or else, what is vulgarly called priggism, a quality for which we possess no genteeler term. He who makes use of x and y when he might say fifty and a hundred, or square and round, because he happens to have arrived as far as biquadratic equations, wishes to show the world that he knows what algebra is, and would fain be thought a mathematician. The mathematician who, similarly, abuses algebraic language, is either a *prig*, or else has been so long versant in the conjuration of *differences*, that he has lost the power of thinking, and the use of his own language. In the last case alone, he is pardonable. The purpose of algebra, of symbols, is abbreviation; it is that we may acquire the power of condensing a long proposition into a small space, so that we may see all its relations at a glance. It is short hand, and no more. If the proposition is not of a nature to require this, from its shortness, or other causes, algebraic symbols serve no purpose but to make a plain thing obscure, and to convey an air of mystery. He who thus uses them, instead of proving his knowledge of algebra, betrays his ignorance, as well as his affectation; he shews, perhaps, that he knows what substitution is, or what is a simple equation; but he shews, too, that he is a mere mechanic in this art.

Now M. Humboldt, as might be expected, has invented a pasigraphy, "a pasigrafia geognostica," since he must also tell us that he has published in Spanish and in Mexico, or has "elevated himself to general ideas" on geognosy by means of α, β, γ, δ. "Cette méthode," also, "est double: elle est ou figurative (graphique, imitative), représentant les couches superposées par des

parallélogrammes placés les uns sur les autres ; ou algorithmique, indiquant la superposition des roches et l'age de leur formation, comme des termes d'une série." And here, also, he takes occasion to tell us that he has published a book "sur l'irritation de la fibre nerveuse," or "Versuche über die gereizte Muskel und Nervenfaser," by which means we know that he writes in German as well as Spanish and French, and understands the nervous system as well as he does economy, geognosy, and astronomy.

"J'ai publié, J'avois, J'ai fait;" I, the great, meets us every where; and we believe, indeed, that there is not one of his books which does not contain an enumeration of all the prior ones, under some pretence or other. We have not the least objection that he, or any man, should write on all subjects, provided he understands them; but when we see this palpable betraying, we draw the natural conclusion, that he does not write from the overflowings of his mind, but because he would be thought capable of writing on every thing.

As to the purpose of this Geognostical pasigraphy, or any purpose to be effected by it, we really cannot explain them, and shall therefore extract one passage, that our readers may form their own judgments.

Si les lettres de l'alphabet représentent ces roches superposées des deux séries,

$$\alpha,\ \beta,\ \gamma,\ \delta\ \ldots\ldots$$
$$\alpha,\ \alpha\beta,\ \beta,\ \beta\gamma,\ \gamma,\ \delta\ \ldots\ldots,$$

la première indique la succession des formations simples et indépendantes: granite, gneis, micaschiste, thonschiefer ou muschelkalk, grès de Königsstein (quadersandstein), calcaire jurassique et grès vert à lignites (sous la craie). La seconde indique l'alternance des formations *simples* avec des formations *complexes :* granite, granite-gneis, gneis, gneis-micaschiste, micaschiste, thonschiefer (pages 67, 69); ou, pour donner un exemple tiré de terrains de transition (page 103), calcaire à orthocératites, calcaire alternant avec du schiste, schiste de transition seul, schiste et grauwacke, grauwacke seul, porphyre de transition.... Dans les formations *complexes*, c'est-à-dire, dans celles qui offrent l'alternance périodique de plusieurs couches, on distingue quelquefois trois roches différentes, qui ne passent pas les unes aux autres dans le même groupe,

$$\text{ou}\ \begin{matrix}\alpha,\ \beta,\ \alpha\beta\gamma,\ \gamma\ \ldots\ldots\\ \alpha\beta\gamma,\ \alpha\beta\delta,\ \beta\alpha\epsilon\ \ldots\ldots\end{matrix}$$

selon que dans le terrain de transition des couches alternantes de granite, de gneis et de micaschiste ; dans le terrain de transition, des couches alternantes de grauwacke, de schiste et de

calcaire, ou de grauwacke de schiste et de porphyre, ou de schiste, de grauwacke et de grünstein, constituent une même formation. Dans le terrain de transition, comme nous l'avons exposé plus haut, le thonschiefer ou le grauwacke seuls ne sont pas les termes de la série. Ces termes sont tous complexes ; ce sont des groupes, et le grauwacke appartient à la fois à plusieurs de ces groupes. Il en résulte, que le terme *formation de grauwacke* n'a rapport qu'à la prédominance de cette roche dans son association avec d'autres roches.

Tous les terrains offrent l'exemple de formations indépendantes qui *préludent* comme couches subordonnés. Si $\alpha\beta\gamma$, ou $\alpha\beta$, $\beta\gamma$ indiquent des formations complexes de granite, gneis et micaschistes, ou de granite et gneis, de thonschiefer et porphyre, de porphyre et syénite, de marnes et de gypse, c'est-à-dire, des formations dans lesquelles des couches de deux et même de trois roches alternent indéfiniment ; $\alpha+\beta$, $\beta+\gamma$, indiqueront que le gneis fait simplement une couche dans le granite, le porphyre dans le schiste, etc. Alors

$$\alpha,\ \alpha+\beta,\ \beta,\ \beta+\gamma,\ \gamma\ \ldots.$$

exprime le phénomène curieux de formations qui *préludent*, que s'annoncent d'avance comme des bancs subordonnés. Ces bancs rappellent tantôt des termes qui précèdent *(roches de dessous)*, tantôt les termes qui suivent *(roches de dessus)*. Ainsi nous aurons :

$$\alpha,\ \beta,\ \beta+\alpha,\ \beta,\ \beta+\gamma,\ \gamma\ \ldots.$$

Les porphyres et syénites grenues du terrain de transition pénètrent dans le grès rouge et y forment des couches subordonnées. Si le gisement des formations de la vallée de Fassa est tel qu'on l'a récemment annoncé (pag. 266), un terme précédent (la syénite) déborde jusque dans le calcaire alpin ou zechstein ; c'est le cas dans la série:

$$\alpha,\ \beta+\alpha,\ \gamma+\alpha,\ \delta\ \ldots.$$

Lorsqu'on veut appliquer la notation pasigraphique jusqu'aux élémens des roches composées, cette notation peut indiquer aussi comment, par l'augmentation progressive d'un des élémens de la masse, surtout par l'isolement des cristaux, il se forme des couches par une espèce de *développement intérieur :*

$$abc,\ abc^2,\ abc^3\ \ldots.\ abc+b.$$

Nous avons préféré, dans ce cas particulier (bancs de feldspath dans le granite, bancs de quarz dans le micaschiste ou dans le gneis, bancs d'amphibole dans la syénite, bancs de pyroxène dans une dolérite de transition), les lettres de l'alphabet romain à celles de alphabet grec, pour ne pas confondre les élémens d'une roche (feldspath, quarz, mica, amphibole, pyro-

ène) avec les roches qui entrent dans la composition des formations complexes.

Jusqu'ici nous avons montré comment, en faisant entièrement abstraction de la composition et des propriétés physiques des roches, la *notation pasigraphique* peut réduire à une grande simplicité les problèmes de gisement les plus compliqués. Cette notation indique comment les mêmes couches subordonnées (le sel gemme dans le zechstein et dans le red marl, §§. 28 et 29; les houilles dans le grès rouge, le zechstein et le muschelkalk) passent à travers plusieurs formations superposées les unes aux autres:

$$\alpha+\mu,\ \beta+\mu,\ \gamma,\ \delta+\mu\ \ldots.$$

Elle rappelle aussi le retour des formations feldsapthiques et cristallines dans les terrains de transition et de grès rouge (Norwége, Ecosse); retour qui est analogue à celui du granite après le gneis et après le micaschiste primitif:

$$\alpha,\ \beta,\ \alpha,\ \gamma,\ \delta,\ \ldots\ldots\ \varkappa,\ \lambda,\ \alpha,\ \beta\ \ldots$$

Les premiers termes de la série reparoissent, même après un long intervalle, après le grauwacke et le calcaire à orthocératites, c'est-à-dire, après les roches *fragmentaires* et *coquillères*.

Now we appeal to our readers, if we have judged M. Humboldt harshly, or whether this is not the very grimace of affectation. If he could reason for one moment, or if he had ever considered the real purpose and use of an arithmetical or algebraical pasigraphry, he would see that he was obscuring what he pretends to illustrate, and increasing labour instead of shortening it. A series summed in this manner, conveys no ideas; and if it is to convey any, the labours of the reader will be, to re-translate all that M. Humboldt has exerted so much useless labour in darkening. We only wonder that he has not given us his geognosy in Hieroglyphics, that he might have tried to persuade us he was the rival of Young and Champollion. One advantage, indeed, we will not deny; and we are only sorry, therefore, that his whole hemispheres were not written in algebra. Each method is equally free of the suspicion of conveying ideas, and the Greek would have been shorter than the French.

Geognosy, indeed, can now be contained in a nut-shell, and by a shorter process than the Iliad was crammed into one. And lest our readers should doubt the possibility of this, we shall give them the whole matter of "the two hemispheres" in two lines. But unless we gave the whole passage, they might suspect us of playing with their credulity, and of jesting with M. Humboldt. Here it is;—

"Pour réunir les principaux phénomènes de gisement des roches dans les terrains primitifs, intermédiaires, secondaires et tertiaires, j'offre la série suivante :

$$\alpha,\ \alpha\beta,\ \beta+\pi,\ \beta\gamma,\ \gamma+\tau,\ \alpha,\ \gamma,\ \delta,\ \alpha,\ \beta,\ \delta,\ o\ \|\ \varkappa^{g}\ \tau'\ \delta\tau',\ \delta',\ \delta'+\pi,\ \gamma,\ \tau',\ \sigma\pi,\ \sigma+{}'\alpha,\ \sigma\pi,\ o\ \|\ \pi\varkappa^{a}+\xi,\ \tau^{a}+\vartheta,\ \varkappa^{n},\ \tau^{m},\ \varkappa^{q},\ \tau^{o},\ \varkappa^{l},\ \tau^{c}\ \|\ \varkappa_{2}^{l},\ \tau^{r}\ \ldots\ldots$$

Il seroit inutile de donner l'explication de ces caractères ; elle résulte de leur comparaison avec le tableau de formation. Je me borne à fixer l'attention du lecteur sur l'accumulation des porphyres (π), sur les limites des terrains de transition et secondaires, sur la position des formations d'euphotide (o), sur les grands dépôts de houille et de lignites (ξ), et sur le retour (presque périodique) des formations feldspathiques, des granites, gneis et micaschistes (α, β, γ) de transition. Comme la notation que je présente ici peut être diversement graduée, en accentuant les charactères, en les réunissant comme des coefficiens dans les formations complexes, ou en ajoutant des exposans, je doute que les noms des roches rangées par séries les unes à côté des autres puissent parler aussi vivement aux yeux que la notation algorithmique."

We must now take our leave of the book and the author. We must be sorry that we have been obliged to write such hard truths, and yet we fear that were the cause to occur again, we should do it again. We have said that it was a justice we owed to others, to shew that M. Humboldt was not the only authority, or the highest, in every subject of science. And it is not fitting or just, that the public, which cannot judge, and which necessarily follows the cry of the day, should measure any man by such an imaginary standard, and set up a false god to worship, to the abasement of all others. It is an age of monopolies; but it is hard that the principle of monopoly should be extended from fish to fame, from tea and porter to geognosy and botany, and to all else which ought to be the common property of the republic (which we hope it will ever continue) of science and letters.

To himself, we might suggest, that, with more accuracy, less *pretension*, better writing, and far less writing, he might have acquired a fair share of permanent reputation; but he has desired to be more than all, and will live to see his honours melt from him as snow before the summer. To retrieve, is perhaps now impossible; but if he shall shew any desire to do so, we will be among the first to allot the praise which, he ought now to know, must follow solidity and accuracy, and can never long follow any thing else.

Art. XVI. MISCELLANEOUS INTELLIGENCE.

I. Mechanical Science.

1. *Steam Engines employed at Glasgow and its neighbourhood.*

	Number of Engines.	Horse Power.
In manufactories	176	2970
In collieries	58	1411
In stone-quarries	7	39
In steam-boats	68	1926
In Clyde iron-work	1	60
	310	6406

Average power of engines, 20.661 horse power.

Cleland on Steam-engines.

2. *Times of the Motion of Solar Spots.*—Mr. Emmett has made many observations upon the motion of solar spots, all of which tend to disprove the opinion that they are hidden and in sight for equal times, and to support the observations of older astronomers, who state the times of appearance and disappearance to be different. Mr. Emmett makes out the time during which they are visible to be $12^{d}\ 8^{h}\ 30^{m}$, and that during which they are invisible to be nearly $15^{d}\ 3^{h}\ 30^{m}$. These nearly accord with the times mentioned by Kirchius, Stannyan, Cassini, *&c.*, and he thinks that imperfection in the instruments, or inaccuracy in the mode of observation, cannot fairly be urged to account for the great difference between these and equal times.—*Ann. Phil. N. S.* ix. 381.

3. *Naphtha Lamps, or Lights.*—M. Joseph Hecker, director of the saltworks, and administrator of the mines at Iruskawetz in Gallicia, has found that naphtha burns much better than oils or other substances in mines, containing bad air, and injures the health of the workmen less. The light of petroleum is to that of colza oil as 1000 : 831.3, and to that of tallow as 1000 : 500.3, supposing that the first burns with a small flame. The quantity of naphtha burnt for lighting the same space is to that of tallow as 1000 : 925.74, and to that of colza oil as 1000 : 673.28 ; coal-tar oil, which is in the same proportion as naphtha, is preferable to it, being less expensive ; oil of bones is that which yields the most brilliant light. In the lighting of mines containing bad air, colza oil and tallow will be extinguished, when naphtha, petroleum, and the oil of bones will still burn ; but naphtha and petroleum are then readily extinguished by slight motion or concussion in the air, the oil of bones being in this case best for use. M. Hecker has found that, in mines where the oxygen had diminished to 18.33 per cent., men were not incommoded. Generally tallow or colza

oil is extinguished in air containing not more than 18.1 per cent. of oxygen, whilst naphtha and oil of bones burn when it contains no more than 13.8 per cent.—*Ann. des Mines*, x. 64.

4. *Method of browning Iron.*—Nitric acid, ½ oz.; sweet spirits of nitre, ½ oz.; spirits of wine, 1 oz.; blue vitriol, 2 oz.; tincture of steel, 1 oz. These ingredients are to be mixed, the vitriol having been previously dissolved in a sufficient quantity of water, to make with the other ingredients one quart of mixture. Previously to commencing the operation of browning a gun-barrel, it is necessary that it be well cleansed from all greasiness and other impurities, and that a plug of wood be put into the muzzle, and the vent well stopped. The mixture is then to be applied with a clean sponge or rag, taking care that every part of the barrel be covered with the mixture, which must then be exposed to the air for twenty-four hours; after which exposure the barrel must be rubbed with a hard brush to remove the oxide from the surface.

This operation must be performed a second and a third time, if requisite, by which the barrel will be made of a perfectly brown colour. It must then be carefully brushed and wiped, and immersed in boiling water, in which a quantity of alkaline matter has been put, in order that the action of the acid upon the barrel may be destroyed, and the impregnation of the water by the acid, be neutralized. The barrel when taken from the water must after being rendered perfectly dry, be rubbed smooth with a burnisher of hard wood, and then heated to about the temperature of boiling water; it then will be ready to receive a varnish made of the following materials: Spirits of wine, 1 quart; dragons' blood, pulverised, 3 drs.; shell lac, bruised, 1 oz.; and after the varnish is perfectly dry upon the barrel, it must be rubbed with the burnisher, to give it a smooth and glossy appearance.—*Silliman's Jour.* ix. 168.

5. *Observations on Calcareous Cements.*—The theory and improvement of calcareous cements have been taken up with much interest in France, *&c.*, and we have at various times given the results of MM. Vicat, John, and others. M. Vicat is opposed in some of his theoretical conclusions by MM. John and Berthier, and he has endeavoured to meet their objections by reference to facts and new experiments. Without entering into the discussion, or endeavouring to convey to others a knowledge of its minute points, we shall take such facts as are brought to light at different times, with the fair conclusions drawn from them; convinced in the highest degree of the importance of the subject, and the advantage which the present investigations must lead to.

M. Vicat states, that two kinds of sand were taken, one white

and entirely siliceous, the other granitic and mixed with basalt; they were washed, digested in muriatic acid, again well washed and dried at a temperature of 212°. June 12, 1822, two specimens of mortar were made as follows:—1. White sand 896 parts, hydraulic lime fresh from the furnace 300 parts: the two mixed with water and in the usual manner in a glass vessel weighing 787 parts, gave, vessel and all, 2630 parts. The water therefore weighed 647 parts, and the fresh mortar altogether 1843 parts.—2. Granitic sand mixed with basalt 896 parts, lime same as above 300 parts; the water used weighed 612½ parts, and the mortar altogether 1808½ parts.

These two mortars, placed in the most favourable circumstances, had in 15 days lost 27 per cent of their weight. On the 4th Feb. 1824, nearly two years after their preparation, they were disintegrated by muriatic acid. The sand of No. 1, washed, dried, and weighed, equalled 892 parts; the loss of $\frac{1}{223}$ was evidently due to the second washing. The sand of No. 2, treated in the same way, amounted only to 883. leaving a loss of $\frac{1}{70}$. This being considerable, 500 parts of the original sand were digested with muriatic acid to imitate the action which had taken place during the disintegration of the mortar, and it was found that in this way it lost $\frac{1}{71}$ part. Hence it may be concluded that the hydraulic lime did not attack either of the varieties of sand.

From these experiments M. Vicat concludes that the solidification of mortar, containing hydraulic lime and common sand, is not the result of a chemical combination. He admits at the same time that the solidification cannot be accounted for by mere mechanical adhesion between the hydrosilicate of lime (in the lime used) and the sand, *i. e.*, a mere entanglement of asperities, "we must therefore admit a molecular affinity without subsequent combination; and thus distinguish two species of adhesion, namely, that which is purely mechanical, such as exists between plaster and wood or stone, and *intimate adhesion* analogous to that which connects most incrustations to the surface on which they have slowly formed.

With regard to the hydraulic mortar made from common lime and puzzolana, M. Vicat thinks the results are very different, and is at issue with M. Berthier on the subject. The induration of these mortars he thinks cannot result from a simple adhesion without subsequent combination; for if combination do not take place, the lime should retain its usual properties, such as solubility, causticity, &c., and the consequence would be that all such water cements immersed in a current would be rapidly decomposed, whereas this does not take place. He also quotes an analysis made by M. John of a terras hydraulic mortar which had been immersed for four years, and in which was found water 24., rains of quartz 33., silica in combination 8., lime with trace of

oxide of iron 32.75, carbonic acid 2.25. Here the lime was neutralized not by carbonic acid, but by silica; and from circumstances M. Vicat thinks it probable that the silica in combination was furnished by the terras. From the whole of the facts it seems that terras, a substance consisting of hard absorbent particles, and being a true puzzolana, cannot furnish carbonic acid to the lime, and yet with the purest lime, even that of shells, it furnishes a good hydraulic cement.

In answer to other doubts of M. Berthier, who inquires why, if common lime acts upon silica, it should not also act upon crude clay, which enters so much more readily into combination than calcined clay, and why also felspar sands act merely as pure siliceous sands, M. Vicat quotes some new facts; from which it appears that substances not baked, not porous, not absorbent, and composed of feeble elements, are competent to the neutralization of common lime, so as to form with it hydraulic mortars. These substances are 1. certain felspar sands or disintegrated granites; 2. the greater number of the friable brown psammites of Bas Brittany. Results have been obtained of this kind by MM. Avril and Payen, and other experiments are in train for their more particular confirmation.—*Ann. de Chimie*, xxviii. 142.

6. *On the Cultivation of the Potato considered as to its produce in potash and in roots.* By M. J. B. Mollerat.—In consequence of the contradictory accounts given by experimentalists as to the produce of potash by the stalks and leaves of potatoes, M. Mollerat undertook certain experiments on the subject. He found in 1818, that the green crop gave its *maximum* of potash immediately before the flowering of the plant, and its *minimum* at the time of maturity. At that time he determined to ascertain the effect of the leaves on the roots, in order to ascertain whether there would be any advantage in collecting the alcali, but had not the opportunity of making the experiment till 1824. A silico-argillaceous soil, rich in alluvium and manure, was then planted with the potato called *patraque jaune*, one of the most productive species, the cultivation being carried on with care.

The following table exhibits the produce in green crop, salts, and sub-carbonate of potash, at the time of each cutting, and of potatoes collected when the plant was mature; the whole calculated on the results of 30 centiares * of ground, and multiplied to represent the produce of a hectare †.

* 1.2 square yards nearly.
† 2 acres 1 rood 35.4 perches.
Note.—A kilogramme is 2 lb. 3 oz. 5 dr.

	Green Crop.	Salts.	Sub-carb. potash.	Potatoes.	OBSERVATIONS.
1st. Cutting immediately before flowering.	kilog. 33383	kilog. 384	kilog. 212	kilog. 4300	Dry crop 0.125 of the green crop.
2nd. Cutting immediately after flowering	33333	311	190	16330	Ditto.
3rd. Cutting one month later.	35700	230	72	30700	A greater product of dry crop.
4th. Cutting one month later.	22300	205	60	41700	Dried on the ground gave a still greater crop than before.

The produce of a fifth cutting did not differ from that of the fourth. The salts which accompanied the potash were not examined. The plant deprived of its leaves by the first and second cutting, had time to produce a few others before the root came to maturity.

According to the above experiments it is evident that there would be no advantage in collecting the potash of potatoes once a year, but perhaps there might be if two crops were obtained in that time. For this purpose the first potatoes must be planted early; and after the first cutting, which should precede the flowering, the earth should be turned up, and a second plantation made, which should have time to yield its crop in a convenient state before the end of the season. It would be useless to endeavour to obtain an abundant crop of leaves on a stem which has already been cut.

This note on the produce of potatoes is terminated by an observation that animal manures improve the vegetation, principally as it affects the leaves, whilst gypsum, mixed with the soil, makes the plant more productive in tubercles.—*Ann. de Chimie.* xxviii. 165.

7. *Magnetism.*—If three lines of stations be assumed in a gun-brig, having no other iron in her than what is commonly employed in the construction of a vessel of this kind, one series of stations being in a vertical plane passing through the principal axis of the ship, and the others in similar planes on the larboard and starboard sides of the ship, at the distance of eight feet from the middle section, it will be found, that when the ship's head bears east, the mean intensities of the three sections will be a minimum, on the poop and forecastle, in the starboard section of the upper deck, and the larboard section of the lower. But in the larboard and middle sections of the upper deck, and the middle section of the lower, the intensities will be the least when her

head is north, and in the starboard section of the lower deck when the vessel is moored with her head north-west. For the proofs of this we refer to Mr. Harvey's paper on *the Distribution and Changes of the Magnetic Intensity in Ships of War*, contained in the second part of the Philosophical Transactions for 1824.

In a ship under the circumstances before mentioned, the intensity of the larboard section will be found to increase 1·26 from the upper to the lower deck, when the vessel's head is moored due north, the magnetic intensity of the earth being represented by 100; but a diminution of the attractive power in the same section, amounting to 12·64, will be found to take place, when the ship's head is directed to the east; and an increase of 7·79 when the bow bears north-west. In the middle section of the vessel, a minute decrease of 0·89 will be perceptible from the upper to the lower deck in her first situation; a rather greater increase of 1·79 when her head is east, and a diminution of 2·43 when it is north-west So also, in the starboard section, the magnetic influence will be found to decrease from the upper to the lower deck, when her head is north, by the small quantity 2·68; on the contrary, a rapid increase of 13·47 when it becomes east; and a considerable diminution, amounting to 10·32, when her head bears north-west.—*Ibid.*

In the middle and starboard sections of the poop; in the three sections of the forecastle; in the starboard section of the upper deck, and in the middle and larboard sections of the lower deck, the stations at which the greatest and least intensities are respectively found, when the bow of the vessel is due north, will be found still to maintain the same property, when her head is moored north-west;—but, in the larboard section of the poop, and the starboard section of the forecastle, the same principle will be only found to hold good when the bow of the ship bears due east. In the middle and larboard sections of the upper deck, however, the points of maximum intensity will be found to preserve their situations in the northern and western positions of the ship; but the points of minimum intensity will be observed to approach the bow, in the latter situation. A similar property with respect to the least intensities will also be found in the middle and starboard sections of the lower deck, when the vessel is moored with her head to the north and east. In these two last positions of the vessel, the points of greatest and least intensity will be found mutually to exchange places at some of the stations; the point of maximum intensity in the northern position of the ship becoming in the eastern that of minimum intensity, and the converse. These singular changes will be found in the middle and starboard sections of the poop; in the middle and larboard sections of the forecastle, and the larboard section of the upper deck. A point in the middle section of the lower deck, two-

ninths of the distance between the two masts of the vessel, estimated from the main-mast, possesses the remarkable property of preserving its minimum intensity in every position of the vessel. —*Ibid.*

In moving the ship's head from one position to another, the motion of the centre of force is in that species of line which geometers denominate *A Curve of double Curvature.*—*Ibid.*

In the Helicon brig, having all her guns and stores on board, the means of the intensities of the three sections, as far as the transverse line of stations, just abaft the main-mast, were found to present a remarkable approximation to equality; being for the

Starboard section 111·92,
Middle section 111·78,
Larboard section 111·08,

the ship's head being moored towards the north;* but the means of the intensities of the former and latter sections, estimated from the before-mentioned transverse line of stations, to a similar line abreast of the foremast, were for the

Starboard section 105·23,
Larboard section 95·67,

the former exceeding the latter in the ratio of 11 to 10.

The intensity in the after part of the ship is much more considerable than in the forward part; for the

Mean intensity of seven stations in the after-part of the starboard section	115·43	Mean 113·55
Mean intensity of seven stations in the after-part of the larboard section	111·67	
Mean intensity of seven stations in the forward part of the starboard section	100·77	Mean * 96·82
Mean intensity of seven stations in the forward part of the larboard section	92·87	

The variations of intensity at different stations in a ship are of a very unequal kind. In some parts, the alteration of a quarter of a point in the direction of a ship's head is productive of a greater change than the variation of an entire point at some other stations.—*Ibid.*

For the Impregnable, a ship of 104 guns, Mr. Harvey found that, in passing from the mean intensity of the poop to that of the quarter-deck and forecastle, there is a decrease amounting to 2·42; from the deck last-mentioned to the main-deck, a feeble increase of 0·03; from the main to the middle-deck, a diminution of 4·43; from the middle to the lower deck, an increase of 4·59; from that to the orlop-deck, a decrement of 6·88; from

* Mr. Harvey has requested us to correct an error, into which he has been led by the inadvertency of a friend, respecting the scharps of the beams of the deck being secured by bolts of *iron*, whereas he now finds them to be *copper*. —See Note, page 328 of the Transactions.

the orlop-deck to the hold, another decrement of 0·09; and from the mean intensity of the hold to the single intensity determined on the kelson, an increment of 14·63. Hence it appears, that the greatest mean intensities are found at the extremes of the series; that the mean results of the quarter-deck and forecastle, and main and lower decks, are very nearly the same; as are also those of the orlop-deck and hold. The mean intensity of the middle-deck is also very nearly a mean, between the mean of the intensities of the three first mentioned decks and of the two latter. The mean of the decks, from the poop to the middle-deck inclusive, is 98·71; and of the lower and orlop-decks, hold, and kelson, 97·84. The mean of all the decks is 98·28, being 1·72 less than the assumed terrestrial intensity.

As a useful practical remark, Mr. Harvey observes, since some difference of opinion has existed on the subject, that during his experiments it has appeared, that the changes and diversities of intensity on board small ships of war are more considerable than those which take place in vessels of a larger class.

8. *Naval Architecture.*—In our Number for January last, we inserted a long and elaborate article on the Comparative Means of Defence afforded by Ships with Square and Curvilineal Sterns; a subject of great interest to our naval readers, and one on which we know they are desirous of having as much light thrown as possible. With this view we present them with an Abstract of a Paper published by the same author, in the second Number of the *Edinburgh Journal of Science.*

Mr. Harvey, the author of the papers alluded to, remarks, "that it may not be generally known that the application of circular sterns to ships of war forms a necessary and important part of the improved system of ship-building, latterly introduced into the public service by Sir Robert Seppings; and that, without the full and perfect application of it to ships of all classes, this excellent system will be rendered, in a considerable degree, ineffectual. Indeed, the necessary and essential connexion between the diagonal trussed frame, the shelf-pieces and thick waterways, and their peculiar adaptation to the form of the circular stern, renders it impossible to separate either from the new system of ship-building, without producing a most important injury to the public service."

Mr. Harvey again observes, "the general and unexceptionable merits of the diagonal system are now universally acknowledged; but the advantages resulting from the shelf-pieces and thick water-ways, and the admirable mode by which the beams are connected to them, by means of coaks and bolts passing through the whole, although clearly understood by those well-informed men who have turned their attention to the inquiry, have, neverthe-

less, not attracted so largely the attention of the scientific world as might have been expected. Their advantages, however, are as unexceptionable as those resulting from the diagonal trusses; and there can be no question but the *longitudinal strength* communicated by the latter would be considerably diminished, if the *lateral strength* afforded by the former were removed, or in any degree impaired. Hence," continues Mr. Harvey, "without an intimate and perfect union of the advantages resulting from both, and which can alone be done *by the perfect application of Circular Sterns*, the separate strengths afforded by the two can never be effectually united."

To this important argument the author adds the no less important one, that, "*in ships with square sterns, the application of the diagonal system of trusses does not produce its maximum effect, nor is the continuity of the shelf-pieces preserved, since the most abrupt termination of them takes place at the quarters, a difficulty entirely removed in the circular form, by the happy introduction of the Ekeing*, and affording a perfect illustration of the term 'internal hoop,' so appropriately applied to them by Sir Robert Seppings."

II. Chemical Science.

1. *Apparatus for exhibiting the Simultaneous Rotation of two Voltaic Conducting Wires round the opposite Poles of Magnets.*—An apparatus of this description was constructed some time ago by Mr. T. Griffiths, and shewn at the London Institution during a course of Lectures on Electro-magnetism, delivered in 1823 by Professor Brande. It consists of two copper wires suspended in the usual manner, and dipping into a shallow vessel of mercury, in which are placed two bar magnets with their opposite poles arranged above its surface. The connexion being established between the battery and the apparatus, the wires revolve round the magnets simultaneously, but in opposite directions. In this form of apparatus the necessity of stopping the single rotation to exhibit its contrary movement by reversing the magnet is avoided.

2. *On the mutual action of Magnetic and Unmagnetic Bodies.*—Extract from the proceedings of the Academy of Sciences of Paris, March 7, 1825.—"M. Arago placed before the Academy an apparatus, shewing, in a new form, the mutual action of magnetic and unmagnetic bodies. In his first experiments he had proved that a plate of copper, or of any other solid or liquid substance, placed under a magnetic needle, exerted on it an action which had for its immediate effect an alteration in the amplitude of its oscillations without sensibly changing their duration. The

phenomenon to-day, before the Academy, was the inverse of the preceding. As a needle in motion is retarded by a plate at rest, M. Arago thought it would follow, that a needle at rest would be affected and drawn by a plate in motion; and, in fact, if a plate of copper, for instance, be made to revolve with a certain velocity under a magnetic needle perfectly enclosed, the needle no longer takes its usual position, but one out of the magnetic meridian, and the more removed as the rotation is more rapid. If the motion is sufficiently quick, the needle itself, at any distance from the plate, will turn constantly in the same direction round the thread by which it is suspended.—*Ann. de Chim.*, xxviii. 325.

3. *On the Voltaic Pile and Current*, by M. A. de la Rive.—M. A. de la Rive, whilst experimenting on the decomposing action of the voltaic pile on solutions, or on water, was led to observe the effect produced by the introduction of metallic plates into the solution, in such direction as to cross, as it were, or intersect the current. The following are the conclusions he has drawn:

1. That one or more metallic plates, placed perpendicularly in a liquid conductor on a line extending from pole to pole, diminishes the intensity of the current which is obliged to traverse them.

2. That this diminution is almost nothing when the current which traverses the plates is very energetic, and produced by a pile of many pairs of plates: but that the intensity of the current traversing the same number of plates diminishes the more rapidly, according as its original intensity is less considerable; and that therefore a very energetic current must be employed to obtain at each pole the same quantity of gas by decomposition when the liquid is continuous, or interrupted by the introduction of plates.

3. That having two currents of the same intensity, the one originally, the other after having traversed one or more metallic plates, the first will diminish much more by the introduction of one plate, than the latter by the introduction of a plate in addition to those it already has passed.

These results were obtained with the use of plates of platina.

At the end of the same memoir, the author has the following further conclusions:—1. That in the present state of science the least improbable explanation that may be given of electrochemical decompositions, is to consider them dependant on the electric currents diffused throughout the liquid conductor.

2. That these currents, of which the existence is demonstrated throughout all the liquid serving to complete the voltaic circuit, are susceptible of certain modifications relative to their intensity, when one or more metallic plates, or imperfect fluid conductors, are placed in their course.

3. That these modifications, somewhat analogous to those sustained by light and heat in similar circumstances, may serve to explain the difference observed between the effects produced by a pile composed of a great number of plates, and those produced by a pile formed of a small number.—*Ann. de Chim.*, xxviii. 190.

4. *Electro-Magnetic Current.*—As the current of electricity, produced by a voltaic battery when passing through a metallic conductor, powerfully affects a magnet, tending to make its poles pass round the wire, and in this way moving considerable masses of matter, it was supposed that a reaction would be exerted upon the electric current capable of producing some visible effect; and the expectation being, for various reasons, that the approximation of a pole of a powerful magnet would diminish the current of electricity, the following experiment was made. The poles of a battery of from 2 to 30 4-inch plates were connected by a metallic wire formed in one part into a helix with numerous convolutions, whilst into the circuit, at another part, was introduced a delicate galvanometer. The magnet was then put, in various positions, and to different extents, into the helix, and the needle of the galvanometer noticed; no effect, however, upon it could be observed. The circuit was made very long, short, of wires of different metals and different diameters down to extreme fineness, but the results were always the same. Magnets more and less powerful were used, some so strong as to bend the wire in its endeavours to pass round it. Hence it appears, that however powerful the action of an electric current may be upon a magnet, the latter has no tendency, by reaction, to diminish or increase the intensity of the former—a fact which, though of a negative kind, appears to me to be of some importance.—M. F.

5. *Electric Powers of Oxalate of Lime.*—Some oxalate of lime, obtained by precipitation, when well-washed, was dried in a Wedgewood's basin at a temperature approaching 300°, until so dry as not to render a cold glass plate, placed over it, dim. Being then stirred with a platina spatula, it in a few moments, by friction against the metal, became so strongly electrical, that it could not be collected together, but flew about the dish whenever it was moved, and over its sides into the sand-bath. It required some little stirring before the particles of the powder were all of them sufficiently electrical to produce this effect. It was found to take place either in porcelain, glass, or metal basins, and with porcelain, glass, or metal stirrers; and when well excited, the electrified particles were attracted on the approach of all bodies, and when shaken in small quantity on to the cap of a gold leaf electrometer, would make the leaves diverge 2 or 3 inches. The effect was not due to temperature, for when cooled out of the

contact of air, it equally took place when stirred; being, however, very hygrometric, the effect soon went off if the powder were exposed to air. Excited in a silver capsule, and then left out of contact of the air, the substance remained electrical a great length of time, proving its very bad conducting power; and in this respect surpassing, perhaps, all other bodies. The effect may be produced any number of times, and after any number of desiccations of the salt.

Platina rubbed against the powder became negative—the powder positive; all other metals tried, the same as platina. When rubbed with glass, the glass became strongly negative, the oxalate positive, both being dry and warm; and indeed this body appears to stand at the head of the list of all substances as yet tried, as to its power of becoming positively electrical by friction.

Oxalates of zinc and lead produced none of these effects.—M. F.

6. *Course of Lightning on and under the surface of Ground.*—On the 28th of May, 1824, the lightning fell upon a tree, about a foot in diameter, standing one or two hundred yards from the house of Ephraim Tucker, in Vernon County. The fluid left few marks of its course down the tree, but tore up the earth very much at the foot of it; and made, in one direction, a furrow, eight or ten feet in length, by following a root that ran three or four inches below the surface, and throwing off the turf in ragged portions. No other effects of the fluid were to be seen near the tree. At the distance of thirty feet from the tree runs a post wall, bounding the meadow, and separating it from the highway; a low wall of small stones, surmounted by two rails, supported by posts standing in the wall. In the highway, near the wall, at this place, begin to appear marks of the passage of the fluid below the surface. The sod, in some places, seemed to be a little raised along the line of its course towards the road. The road here is formed in the middle of a highway, sixty-six feet wide, as turnpike roads are commonly built, by raising a path twenty feet wide, or more, with earth, taken from the edges of it, which are thus sunk so as to form ditches, commonly four or five feet wide, and one or two feet deep. From the wall to the ditch, and across the road and ditch, the fluid certainly passed under ground, and almost in a straight line. Before reaching the ditch it passed under a thick bunch of bushes, forming a matted bundle of roots and earth, two or three feet in diameter, and raised a little above the adjoining surface. In coming from beneath this cluster of bushes, which stood near the ditch, the fluid came so near the surface, as to throw off considerable lumps of earth from the side of the ditch, and raise and crack the surface all along its course across the bottom of it. It does not seem to have come out of the ground here, but continuing under ground, it went

square across the road, cracking and crumbling the surface very much, eight or ten inches in width, and raising a convex ridge from two to four inches high; a ridge exactly resembling, except in size, those produced by a common species of mole passing near the surface. The fluid seems to have passed the road ten or fifteen inches deep. The soil is here somewhat gravelly, and the road trodden very hard. In approaching the ditch on the other side of the road, the fluid threw off from the edge of the road a large cake of hard earth, eight or ten feet long, and from one to four wide. This was not entirely broken up, but was pushed a little forward, broken into large masses, and some of it crumbled. The fluid was here divided into three portions, and took as many different directions. In two of these directions it left marks of violent action along the surface. The third portion plunged under a very thick and matted clump of roots of small bushes, and came out on the opposite side, at a distance of ten feet, and in ten or fifteen feet more spent itself. The only circumstance that can be thought peculiar in this case is the passage of the electric fluid for such a distance under the surface of the earth, and that without following any such substances as commonly guide its course there, as roots, stones, &c. The fluid seems not to have been guided at all by any attracting substance, but to have been carried forward nearly in a straight course by a momentum it had received through a medium opposing the most powerful resistance, a medium in which it is commonly supposed to be almost immediately dissipated and lost. The fluid certainly passed thus from the wall to the second ditch, a distance of nearly fifty feet, and after passing this ditch one portion of it passed ten feet through, or under, a very tough clump of roots. Without any difficulty I thrust a stake, six or eight feet long, its whole length beneath a clump of bushes, along the course of the fluid, while my strength was insufficient to make it penetrate at all in any other direction. Along the whole fifty feet the evidence of its having passed was indisputable. How the fluid passed through the thirty feet from the tree to the wall, may, perhaps, not be thought quite so certain, as it left no signs of its passage above ground, and no indubitable ones could be discovered below by thrusting down a staff. But, for myself, I cannot doubt the first part of its course was similar to the latter part; but passing below a thick and strong turf, and, perhaps, a little deeper, its course could not be so easily traced. If the fluid did not pass under ground the first part of its course, it must have come out of the ground a few feet from the tree, leaped thirty feet through the air to the wall, and without leaving any trace of its influence on the posts and rails, or displacing the small stones which composed the wall, sunk quietly down through the wall to its foundations, and then gone off as before described at right angles to

the wall, in the direction of a line from this spot to the tree, I cannot doubt that it passed the whole way from the tree under ground.—*Described by* Professor Kelloy. *Silliman's Jour.* ix. 84.

7. *Cold produced by the combination of Metals.*—Some striking examples of depression of temperature during the liquefaction of metals has been pointed out by Dobereiner. According to him fusible metal is a compound of one atom of lead 103, one atom of tin 59, and two atoms of bismuth 142; or it consists of an atom of the combination of bismuth and lead, and an atom of the combination of bismuth and tin. It becomes fluid at a temperature of 210°. The melting points of these alloys of bismuth and lead, and of bismuth and tin, in a separate state, are respectively between 325° and 335°, and between 268° and 280°.

If 118 grains of tin filings, 207 grains of lead filings, and 284 grains of pulverised bismuth, (the constituents of fusible metal,) be incorporated in a dish of calendered paper with 1616 grains of mercury, the temperature instantly sinks from 65° to 14°. He even thinks it would fall so low as the freezing point of mercury, could it be performed in temperatures somewhat under 32°.

In like manner when 816 grains of the amalgam of lead, (404 mercury + 412 lead,) were mixed in a temperature of 68° with 688 grains of the amalgam of bismuth, (404 mercury + 284 bismuth,) the temperature suddenly fell to 30°., and by the addition of 808 grains of mercury, (also at 68°,) it became as low as 17°, the total depression amounting to 51°.—*Ann. Phil.* N. S. ix. 389.

8. *Light produced during Crystallization.*—Dobereiner states, that a splendid instance of light produced during crystallization was observed by M. Buchner, of Magonza, during the sublimation of benzoic acid, previously mixed with pulverized carbon. The sublimation was carried on in a tall glass cylinder upon a stove, and when it had well began there appeared an uninterrupted succession of sparks, continuing for half an hour, and which ceased only when the cylinder was removed from the stove.

Dobereiner states, that he has reason to believe those salts, containing no water of crystallization, are especially powerful in producing light during their crystallization.—*Gior. di Fisica,* vii. 470.

9. *Colour of Glass as affected by Light.*—An effect is noticed at p. 164, vol. xvi, of this Journal, due to the action of light upon glass, containing oxide of manganese, it being found that glass of a pale colour, or even colourless at first, by long exposure to solar rays, became pink, whilst portions of the same glass, retained in dark situations, were apparently unaltered. It occurred

to me that advantage might, in some cases, be taken of this action for the removal of colour in glass. The oxide of manganese is added partly to neutralize the green tint which otherwise would, in some cases, be conferred by iron; but notwithstanding this, some glass to which oxide of manganese has been added, still possesses a green tint. Many specimens of plate glass are thus coloured, though all contain manganese. On exposing such glass to solar light for some time, (from May 7, 1824, to June 1825,) whilst parts broken off from the pieces were retained in the dark, it was found that the green tint was very much altered, and the glass considerably ameliorated in colour, an effect which I attribute to the solar light acting upon the manganese; and even, at this late period, with respect to the glass enabling it to perform the office for which it was at first introduced into the glass pot.—M. F.

10. *Curious change of colour in Oxides of Cobalt and Zinc.*—It has been observed, that when a mixed solution of zinc and of cobalt is precipitated, so as to furnish a mixture of the oxides, and this mixture be well washed and dried, it forms a white powder, which, when heated in a close vessel, loses water, and becomes of a beautiful green colour, though the oxide of cobalt does not amount to above one-sixth part of the whole. When well prepared it is probable that this compound may be found to be a very useful pigment.

11. *Nature of Colour in Mineral Productions.*—A correspondent in the Annals of Philosophy was led to doubt of the constant necessity of any specifically colouring ingredient in minerals, from observing that crystallized felspar exhibits a decided red colour, though analysis points out no substance to which it could be owing; and also from observing that the mineral, after exposure to a strong heat, has the colour entirely destroyed, a colourless glass being obtained. By accident he obtained an artificial mixture of bodies, which exhibited a property of this kind, becoming coloured by particular treatment, though containing nothing to which that colour could be particularly referred. Lime, alumina, silica, soda, and boracic acid, were coarsely mixed together, and exposed to a strong white heat, which produced a semi-vitrified mass of a pure white; a portion of this was finely ground, and after exposure to a low red heat, not above that of melting silver, was found, with surprise, to have assumed a red colour; which colour, with the increase of heat, entirely disappeared, and the substance assumed, at last, as pure a white as it possessed after the first fusion. A lump of the original mass underwent no change

of colour at the same heat, and it was uniformly found, after several trials, that the depth of the colour depended on the fineness of grinding. Nitre destroyed the colour in the fire.

Specimens were sent to the editor, according to whom the pulverised portion, exposed to a low red heat, was of a peach blossom colour; another portion, exposed to a higher temperature, pale bluish lilac; a third portion, exposed to a moderate white heat, was a slightly greenish white enamel; and a fourth portion, mixed and heated with one fifth nitre, was like the second of the above.—*Ann. Phil.* N. S. ix. 432.

12. *On the means of Testing for Iodine.* By M. Balard.—Notwithstanding the delicacy of the test for the presence of iodine afforded by the use of starch, its value is considerably diminished by the facility with which various substances interfere with its action; and this is especially the case when any of those bodies which, either alone or with water, yield hydrogen are present: the iodine becomes hydroidic acid, and the blue colour either disappears, or is not formed. Sulphurous acid and sulphuretted hydrogen, substances almost constantly produced by the incineration of organized bodies which contain earthy or alkaline sulphates, are especially capable of producing this effect.

Among the various means of obviating this inconvenience is the use of chlorine, and during his experiments on the products of the Mediterranean, M. Balard has been led to apply it in the following manner:—After having mixed the liquid containing the iodine with the starch and the sulphuric acid, a small quantity of aqueous solution of chlorine is to be added, which from its lightness may be made not to mix with the mixture, but float on the surface. At the place, however, where they touch, a blue zone will be perceived, which, however feeble it may be, is readily seen by contrast with the neighbouring limpid liquids; if a part of the mixture be slightly agitated, the blue colour will be developed where the two solutions are in contact; but if the whole be mixed, it will entirely disappear, if the chlorine be in excess.

By means of this test thus applied, M. Balard has found iodine where it could not before be observed, as in the various marine mollusca, testaceous or not, such as the *doris*, *venus*, *oysters*, &c., many polypi and marine vegetables, the *gorgonia*, the *zostera marina*, &c., and especially in the mother water of the salt pans fed by the Mediterranean. The small quantity of iodine which is found in sea-water, he believes, from the mode of action of the test, to exist as a hydriodate.

M. Balard expresses his conviction that a process of this kind will save chemists the trouble of many fruitless trials, and lead to decided and positive results, where otherwise none would be obtained.—*Ann. de Chimie*, xxviii. 179.

13. *Presence of Iodine in Sulphurous Mineral Waters.*—In consequence of the great advantage found from the use of the sulphuretted water of Castelnovo d'Asti, in the treatment of goitre, and other disorders of the glandular system, Professor Cantu, of Turin, was induced to search in it for iodine; and though he failed at first, yet encouraged by the results of M. Angelini, who found that substance in the waters of Vogera and Sales, he renewed his attempts, and succeeded in the following manner:—

After having evaporated some of the water to dryness, he treated the residue with alcohol, which dissolved the salt of iodine and the deliquescent chlorides. The solution was evaporated, the residue dissolved in a weak solution of starch and a little chlorine added, the liquid acquired the same fine blue colour, produced by adding to an aqueous solution of iodine, a few drops of solution of starch. Instead of employing alcohol, the mother water left by evaporating the water almost to dryness, may be treated directly with starch and chlorine. M. Cantu is led to believe that iodine exists in all the sulphurous waters which contain chlorides. When present it is evidently in the state of iodide.

Various mineral waters, not sulphurous, and particularly that of Echaillon, in Savoy, which yields a twelfth its weight of common salt, and is considered a powerful remedy against goitre, do not yield any trace of iodine.—*Ann. de Chimie*, xxviii. 221.

14. *On the advantageous Preparation of Ammoniacal Compounds.*—Ammonia, carbonate of ammonia, and most, if not all ammoniacal preparations, are obtained by treatment of the muriate of ammonia in various ways with other substances. The editor of the *Annales de Chimie*, observing on these processes, states upon the authority of M. Payen, that more economical processes may be adopted. For this purpose crude sulphate of ammonia is to be highly dried, so as to have one part of the contaminating empyreumatic oil volatilized, and the other rendered insoluble by carbonization. When the heat has been well managed and rendered very equable throughout the mass, the sulphate is not sensibly altered, whilst not the smallest quantity of soluble oily matter remains, so that the filtered solution of this torrified sulphate is colourless; but great care must be taken that the heat be not too strong, or merely a carbonaceous residue will be obtained. Sometimes from fear of too much heat it is hardly raised enough; the sulphate must then be purified by making a hot solution, filtering and allowing it to crystallize as it cools. The crystals when drained, washed, and dried, may be employed in the same way as muriate of ammonia for the preparation of ammonia or its carbonate.

It is requisite for the success of these operations that the sulphate of ammonia, and the chalk or lime, should be pulverized as finely as possible, and intimately mixed. A little water, or weak

ammonia, is to be added to the mixture of lime and sulphate before distillation; for the subcarbonate the chalk is to be dried and sifted.

The economy of this mode may be readily appreciated by comparing the commercial value of sulphate and muriate of ammonia, and the proportions of ammonia which they contain. White sublimed sal ammoniac is worth 4*f*. 70c. per kilogramme in commerce: it contains 31 ammonia and 69 of acid. The crude sal ammoniac sells for 4*f*. 50c. per kilogramme; crude crystallized sulphate of ammonia sells at 80c. the kilogramme; torrified at 1*f*. the kilogramme; white and dry at 1*f*. 20c. In this state it contains 29 ammonia, and 71 of acid.—*Ann. de Chimie*, xxviii. 171.

15. *Test of the presence of Muriatic or Nitric Acid, or Salts of these Acids.—For Muriatic Acid, or Muriates.* Put pure nitric acid into a porcelain capsule, and throw to the bottom of it a very minute quantity of finely-divided gold, precipitated from its solution by sulphate of iron; put into the acid a minute fragment of any muriate, by degrees the gold will be surrounded by a light yellow tint, a certain sign that it is attacked. A fiftieth of a grain of a muriate may be detected in this manner.

For Nitric Acid or Nitrates. Operating in an inverse manner, the presence of a nitrate in a soluble salt may be ascertained. The gold should be put into muriatic acid as pure and as colourless as possible, and then a fragment of the salt, supposed to contain a nitrate, added; sometimes this experiment requires many hours for its completion, if there be but a small quantity of nitrate present.

This variation of Dr. Wollaston's test, may at times be usefully employed.—*Ann. de Chimie*, xxviii. 36.

16. *New production of Anhydrous Sulphuric Acid.*—Professor Gmelin has observed the formation of anhydrous or fuming sulphuric acid, from common hydrated sulphuric acid, in a new and interesting manner.—6 lbs. 14½ oz. of English oil of vitriol, not at all fuming, and of a specific gravity of 1.8435, were heated in a retort, the temperature of the air round the receiver, *&c.*, being 32° Fahr., and the acid never boiling. Four ounces distilled over, having a strong odour of sulphurous acid; these were removed, and a clean receiver applied: acid then came over quite destitute of smell, but when 8 ounces had been received, the receiver, before quite clear, became filled with vapours. It was removed and another applied, and cooled by ice: there condensed an acid partly transparent and crystalline, and a portion of solid acid was found in the retort neck. This acid was very fuming, it remained solid at 12° R. (59°F), and had no smell of sulphurous acid. It formed with sulphur a green compound, and a little sulphurous acid was disengaged: the green mass in contact with wa-

ter evolved great heat, sulphurous acid was formed and sulphur evolved. The solid acid in contact with water became diluted, but no sulphurous acid gas was produced. The specific gravity of the acid left in the retort, now sensibly fuming, was 1.8502 at 18° R. (62° F.) ; the acid which distilled over was of specific gravity 1.4309, at 11.5.° R. (58° F.)

This fuming acid was formed only in the first half of the third day, fire having been kept in the furnace from seven o'clock in the morning till nine o'clock in the evening, during the two first days; and its formation could not be perceived for a longer period than half an hour. The distillation had been carried on therefore very slowly, and without great care the formation of the anhydrous acid might easily be overlooked. Its formation is thus explained: that by a certain process of concentration of the hydrated sulphuric acid, obtained by the previous long and slow distillation, part of the acid takes water from another part, and is volatilized, leaving a portion so concentrated as at some particular point to resolve itself into two parts, one the anhydrous acid, the other acid of the usual degree, or nearly so, of concentration ; the superior volatility of the anhydrous acid, allows it to separate and distil over at the particular time of decomposition: and thus these changes seem effected.—*Brewster's Journal.*

17. *Hygrometrical Indications by Sulphuric Acid.*—M. A. de la Rive has applied the heat produced by the union of sulphuric acid and water, as a measure of the quantity of water in the atmosphere, and in his manner of applying it, has thus constructed a new kind of hygrometer. A very delicate thermometer, with a small bulb, being dipped in sulphuric acid, slightly shaken, and then exposed to the air, will exhibit an elevation of temperature up to a certain point, after which the mercury will fall. M. de la Rive states that the elevation of temperature is proportional to the quantity of water in the atmosphere. Thus supposing the instrument at 12° (53°.5 F.) when plunged into the acid, and that by exposure to the air it rose to 25° (78° F.) *i. e.*, 13½° (24.5 F.), and that put into a jar containing air of extreme humidity, it rose to 27° *i. e.*, a change of 15° (27°F.) the ratio of 13½° to 15°, will express that of the tension of the aqueous vapour existing in the air, to the tension of the aqueous vapour in an atmosphere saturated with water.

For the application of this principle it is requisite that the number of degrees that the thermometer will rise in air saturated with water, at all temperatures, should be ascertained. It is proposed that these should be set down on a thermometer opposite to the temperature of the air in which they were obtained; then on making an experiment, the number of degrees which the thermometer rises in air at any temperature is to be divided by the

number of degrees against that temperature on the thermometer, as obtained in an atmosphere saturated with water, and the result expresses the ratio of the water in that atmosphere to a saturated atmosphere considered as unity.

This is a curious application of the heat occasioned by the union of water and sulphuric or other acids; but of course has no pretensions to the accuracy of that philosophical instrument Daniell's Hygrometer, nor has any other instrument with which we are acquainted.—*Bib. Univ. April* 1825.

18. *Exposure of Iron to Air in high Regions.*—In one of the excursions made by M. Zumstein and others to the summit of Monte Rosa, an iron cross was fixed upon it and left there. This was in August 1820. M. Zumstein again ascended the mountain in August 1821, and after great risks reached the summit and the cross. The latter was found not at all rusted, but had taken the colour of bronze. The barometer was at 16 inches 4.2 lines. The temperature of the air was — 0.5 R (21°F.), and by calculation the height was obtained as 14,086 feet. Water boiled at 68.38° R. (185.°8 F.)—*Bib. Univ*. xxviii. 65.

19. *Applications of Chromate of Lead in the Arts.*—In a paper published in the *Annals of Philosophy*, *N.S.* ix. 303, Mr. Badams describes the results of certain experiments on the composition of the scarlet or sub-chromate of lead, and the yellow chromate, and highly recommends the use of the former, as a colour either in painting, or calico-printing.

He prepares the red chromate by boiling the yellow chromate with potash, a process devised by Grouvelle. Upon digesting 100 grains of this scarlet chromate in diluted acetic acid for half an hour, and constantly stirring, the substance became of a pure yellow colour, and was found when washed to weigh only 60 grains, acetate of lead remaining in solution. On the contrary, grinding 60 grains of yellow chromate with 40 grains of oxide of lead with the addition of small quantities of hot water from time to time, the red sub-chromate was again produced. After some further experiments, Mr. Badams gives the composition of the *yellow chromate* *Red chromate.*

Chromic acid	1 atom	19.02	- -	1 atom 19.02
Oxide of lead	1 ——	40.98	- -	2 atoms 81.96

Having made many experiments upon the properties of the scarlet chromate as a fast colour in calico-printing, and a durable pigment for artists in oil and water, he states that with respect to the first object, the practice of the calico-printer in making the yellow chromate fast, will teach him how to apply the red chromate; the colour being given to him by nitrate of lead and an alkaline solution of chromate of potash. He may accumulate be-

sides some insoluble salts of lead in the pores of the cloth to give stability to the tint, but in all cases the colours must be heightened at last by passing them through boiling water.

The scarlet sub-chromate is extremely beautiful when ground up with oil, and possesses great body as a pigment. It is not degraded in its hue like vermilion by admixture with white lead; it mingles with other colours, and shews no signs after long exposure, of any change by time. As a water colour it has not been tried sufficiently to authorize a positive declaration that it will not blacken; but several pieces of cards and thin paper painted with it, and hung up in situations likely to influence the colour of salts of lead have not in some months perceptibly diminished in brightness. Mr. Badams observes that should it succeed, no tint would be a more desirable accession to the pallet than a bright and permanent scarlet or scarlet-orange.

20. *Cadmium in Ireland.*—Upon examining a slag produced during the smelting of galena, Mr. Apjohn discovered cadmium in it in considerable abundance, amounting to as much even as 5 per cent. This is stated to be the first time the metal has been found in Ireland, but there is no intimation from whence it came or where the galena works are situated. The slag was pulverized and digested in nitric acid, the filtered solution was precipitated by sulphate of soda, again filtered, treated with caustic ammonia in excess, the iron separated removed by the filter, the clear solution evaporated, and the oxides gradually thrown down. These dissolved in muriatic acid, were treated with carbonate of ammonia in such excess as to dissolve any zinc at first thrown down. An insoluble part was obtained, which being well washed, was dissolved in muriatic acid, the solution introduced into a platinum capsule with a piece of zinc in Dr. Wollaston's manner, and next day the cadmium was found lining the crucible, and easily freed from the supernatent solution by washing. The metal when examined had all the characters of cadmium.—*Dublin Phil. Jour.*, i. 73.

21. *Explosion of fulminating Powders.*—There is an old notion that fulminating powders act more powerfully downwards than in any other direction. We take the opportunity of remarking on it here, in consequence of an essay on this subject by M. Brianchon, which is noticed at considerable length in the *Bib. Universelle*, xxviii. 293, and from having heard many persons express an opinion of the same kind. The opinion has arisen no doubt from the difference of effect observed, when these preparations, such as fuminating gold or silver, have been exploded on a horizontal surface, and compared with the explosion or rather perhaps inflammation of gunpowder in the same circumstances. A very great

difference of effect is observed upon the surfaces which have supported the powders, that is to say, in the direction downwards; and this has been admitted as if the difference existed exclusively in that direction, merely because the experiment has not been made with the resisting plane in other positions. It will be found, however, that if a portion of fulminating silver be attached to a metallic or other plate, that when exploded, whatever the position of the plate, the effect upon it will be sensibly equal. All the differences in the phenomena presented by these powders and gunpowder, depending upon the greater rapidity of chemical action in the former than in the latter.

M. Brianchon's theory of the supposed force of these powders in a direction almost exclusively downwards, is, that being compounds, deficient in oxygen, but resolved by heat in the air into compounds containing much oxygen, the vapour of the substance at the moment of explosion seeks in the atmosphere for the oxygen it wants, abstracts it as it were by powerful suction, and causes a precipitation of the columns of air on to it. M. Brianchon seems to think that the columns of air can only move downwards in this case, and he illustrates his opinion by the old experiment of breaking a bladder on the air-pump over a receiver exhausted beneath it. There is no reason, however, why the air at the sides or beneath should not also rush towards the point of action, or to express it more generally, why, when either expansion or contraction to any extent takes place in the atmosphere from a single point, whether from gunpowder, fulminating power, or what not, the effect should not be alike in every direction, seeing that the air is a fluid so eminently elastic, mobile, and uniform.

In consequence of his particular views, and the cheapness of the materials of which common fulminating powder is made, namely sulphur, carbonate of potash, and nitre, M. Brianchon has proposed its use as a mechanical agent acting from above downwards, for the crushing, burying, or throwing down bodies of large dimensions. He intends proposing its use in war time for the destruction of bridges, and means to request that experiments be made on this subject.

22. *Moretti's fulminating Acid.*—On occasion of the late experiments made by MM. Liebeg and Gay-Lussac, on fulminating compounds, *&c.*, the Giornale di Fisica has republished the account of an acid also possessing detonating properties, prepared as far back as 1808 by Professor Moretti, from indigo by nitric acid. We make the following brief extracts of the experimental results which it contains: 14 parts of nitric acid of specific gravity 1.430 and 1 part of Guatimala indigo were distilled together; about a fourth part of the new acid was obtained with a small

quantity of yellow bitter substance. Repeated washing in alcohol and distilled water separated this substance, and the acid dissolved in boiling water and crystallized by cooling was obtained pure. The acid is always of a yellow colour, of a bitter taste, crystalline, soluble in water, and when placed upon a hot coal deflagrates something like nitre.

Combined with potash, soda, ammonia, baryta, and lime, it formed neutral salts, all of which are described as fulminating.

In consequence of the suspicion that it might be benzoic acid, Berthollet, Fourcroy, and Vanqueline, having said, that benzoic acid is formed by the action of nitric acid on indigo, it was compared with that substance as to solvent power, &c., and found to be essentially different.—*Giornale di Fisica*, vii. 414.

23. *Researches on a new Acid universally diffused through Vegetables, by* M. H. Braconnot.—I first obtained this acid from the tubercules of the dahlia and artichoke, whilst engaged in their analysis, but ignorant of most of its properties, I neglected to insert it as a constituent principle. Some time after, whilst examining the roots of cultivated celery, I found the same principle, and observed its acid properties. It has, since then, occurred so frequently, that I have not met with a single vegetable or succulent root without observing its presence.

I have found it in the roots of the turnip, carrot, phytolacca, (*phytolacca decandria*), scorzonère (*scorzonera hispanica?*), piony, phlomede (*tubereuse*) in the roots of patience (*Rumex patienza*), and of filipendule (*Saxifraga rubra*), where it is united to a colouring principle; in bulbs, the onion; in the stalks and leaves of herbaceous plants; in the corticle layers of all trees previously deprived of the coloured external bark, from whence it may be obtained in great abundance, sometimes united to a red colouring matter, as in the cherry-tree, maple, nut-tree, at other times colourless nearly, as in the elder-tree; in sawdust of the maple; in apples, pears, prunes, cucurbitaceous fruits, and, without doubt, in all other fruits; in grain.

Before describing the properties of this acid, I should state, that it appears to me very analogous to, if not identical with, the principle so little known under the vague term of jelly. However that may be, it is easily obtained from all parts of plants. If roots containing starch be operated upon, such as those of celery and carrot, they are to be reduced to pulp by rasping, the juice expressed, the residue boiled with water slightly acidified with muriatic acid, then washed, and afterwards heated with a very dilute solution of potash or soda; a thick mucilaginous liquid results, slightly alkaline, from which muriatic acid separates the new acid in the form of an abundant jelly, which should then be well washed. In this state, it is almost colourless, especially when from colourless vegetable substances: it has a

sensibly acid taste, and applied to litmus paper, distinctly reddens it, though containing no foreign acid.

It is scarcely soluble in cold water, but a solution may be obtained, sensible to re-agents. Boiling water has a stronger action; the filtered liquor is colourless, deposits nothing on cooling, and barely reddens litmus. Alcohol coagulates it into a transparent jelly, colourless as ice; so also do all metallic solutions, lime water, baryta-water, acids, muriate and sulphate of soda, nitrate of potash, &c. This acid appears to be held so feebly in aqueous solution, that the solution of a little sugar is sufficient to make the greatest part of the liquor coagulate.

Dried in a capsule, it appears in transparent films having no adhesion to the vessel. When dry, it scarcely swells in cold water; dissolves in small quantities in boiling water, and offers the phenomena just described.

The acid precipitated from its potash combination by muriatic acid, when distilled, did not swell up, and gave a product containing much empyrumatic oil, but no ammonia, nor muriatic acid; much charcoal remained.

Diffused through water, and aided by warmth, it disengages carbonic acid from its alkaline combinations.

It forms a very soluble salt with potash, which may be obtained in the state of transparent jelly, by adding weak alcohol, which removes the excess of alkali, and colouring matter, if there be any. This jelly, washed on a cloth with alcoholized water, pressed, and dried, is a neutral combination, which swells and dissolves in water, and leaves, upon evaporation of the liquid, a transparent mass, full of cracks, and resembling gum-arabic in appearance; it has so little disposition to adhere, that the slightest friction removes it entirely from the capsule. The taste of this salt is insipid; put upon a plate of red-hot iron, it swells excessively, leaving a deep-brown residue, soluble in water, and having the character of ulmin, united to potash. Exposed to the flame of a taper, it burns, producing delicate filaments, which project out from the mass like vermicelli. These filaments fuse into globules of sub-carbonate of potash.

This salt, in aqueous solution, is gelatinized by alcohol, sugar, muriate of soda, acetate of potash, and other neutral salts. All earthy and metallic salts decompose it by double affinity. The acids unite to the alkali, and separate the acid as a jelly. One hundred parts of the neutral combination burnt in a platina crucible, left a quantity of sub-carbonate of potash, which, heated to redness with sulphuric acid, gave 28 parts of sulphate of potash, from which it would appear that the salt was formed of

Acid . . .	85
Potash . . .	15
	100

Let it not be supposed, that this salt will be useless; I am persuaded that it will have many applications in the art of the confectioner. It is, indeed, remarkable, that so small a quantity of the combination can communicate the property of gelatinizing to such large quantities of sugared water. I dissolved one part of this salt obtained from the turnip root in a certain quantity of warm water; sugar was then dissolved in the liquor, and finally, a very small quantity of acid added; an instant afterwards, the whole became a trembling gelatinous mass, weighing 300 parts. I have in this way prepared aromatic jellies, perfectly transparent and colourless, and very agreeable to the taste and sight. By colouring rose-water with a little cochineal, I have also prepared jelly of roses, of an exquisite taste.

The same acid added to weak solution of ammonia, forms also a soluble compound, which, evaporated to dryness, leaves a neutral insipid salt, something like gum, which separates from the evaporating dish in large plates, like mica. Its solution coagulates with different substances in the same manner as the preceding. I dissolved one part of this combination in 100 parts of water, and added alcohol, which separated a transparent jelly; this, drained on a cloth, weighed 110 parts. This salt will answer, like the preceding, in the preparation of jellies; and indeed is more advantageous, since it may be readily obtained in the neutral state.

The other combinations of this acid are almost all insoluble, and may be obtained by double decomposition.

Concentrated sulphuric acid appears to have but little action when cold on this acid: by means of heat it produces sulphurous acid and ulmin. Nitric acid distilled to dryness with it leaves a residue which with water yielded oxalic acid, and a white powder, which was partly dissolved by ammonia, and left oxalate of lime. An acid added to the ammoniacal solution separated a granular sediment, crystalline, acid, and having the characters of mucous acid; heated in a glass tube it blackened, fused, swelled up, and produced an acicular crystalline sublimate.

Such are the principal properties which I have observed in this acid. It may be presumed that a principle thus universally spread over all vegetables has an important part to play, and merits the serious attention of physiologists. I am very much disposed to think that it is the same thing as the *Cambium*, or organizable substance of Grew and Duhamel, and which appears, as is known in gelatinous drops, wherever new developements are forming.

I shall endeavour in the spring to ascertain whether my conjecture is well-founded. In the mean time I propose the name of *pectic acid*, from πηκτις coagulum, to distinguish it from other bodies of the same class.—*Ann. de Chim.* xxviii. 173.

24. *Conversion of Gallic Acid into Ulmin by Oxygen Gas.*—According to Dobereiner, when a solution of Gallic acid in liquid ammonia is placed in contact with oxygen gas it gradually absorbs as much of the latter as is requisite to convert the whole of its hydrogen into water: 100 parts of the acid absorb 38.09 parts of oxygen. In the ordinary atmospheric temperature, the absorption is complete at the end of from 18 to 24 hours. While this change is going on, the solution becomes intensly brown coloured and opaque; and on the addition of muriatic acid, it lets fall a pale-brown coloured flocculent substance, which possesses all the characters of ulmin. From this experiment, Dobereiner considers it probable that ulmin consists of an atom of oxide of carbon (= 12 carbon + 8 oxygen*) in combination with an atom of water (= 1 hydrogen + 8 oxygen): it is certain, at least, that if the details of the experiment be accurate, the constituents of the Gallic acid, (according to Berzelius's analysis) taken in conjunction with the absorbed oxygen, are resolvable into this simple ratio of atoms.

Gallic acid, prepared by Scheele's process, even after having been crystallized from absolute alcohol, absorbs considerably less oxygen than the sublimed acid, and it appears, therefore, to be still contaminated with tannin, or with some other foreign admixture.—*Ann. Phil., N. S.*, ix. 390.

25. *Presence of Oxalic Acid in the mineral Kingdom, in enormous quantities, in certain plants, and on its advantageous preparation; by* M. H. Braconnot.—Whilst botanizing, some time since, M. Braconnot collected a yellowish matter from a cavity in a calcareous rock, which, examined at the time, did not effervesce with acids, or contain any notable quantity of lime; and was found, some time after, by treatment with carbonate of soda, to contain a considerable quantity of oxalic acid. Not supposing it could be a vegetable production, inasmuch as the rock was steril, and yet surprised at it, as a mineral body, he examined the place again, and finding certain crustaceous lichens growing there, he was again struck with an observation he had previously made, of the enormous quantity of lime in some of these plants, amounting to as much even as 18 per cent. The analysis referred to is in the *Annales de Chimie.*, vi. 133; and there, the 18 parts of lime are considered as united to 34 parts of an unknown vegetable matter. This was now found to be oxalic acid, and in smaller quantity (29.4).

The following experiment will readily account for the presence of oxalate of lime in inorganic natural substances. Some of the common *variolaire* was collected from the surface of an old

* This oxide of carbon is of course merely hypothetical.

diseased beach-tree, many hectogrammes (each above $3\frac{1}{2}$ ounces) being collected with facility. This lichen, when pulverised and sifted, appeared as a white earthy powder: 100 parts, by drying, lost 5 parts. Being boiled with a solution of 100 parts of crystallized carbonate of soda, and filtered through a cloth, the solution was collected, and saturated by nitric acid, which caused no effervescence. Acetate of lead being added, a dense white precipitate fell, and this, washed and decomposed by sulphuric acid, gave a solution almost colourless, which, by crystallization, furnished fine crystals of oxalic acid, amounting to $15\frac{1}{2}$ parts, notwithstanding loss of matter. The residue left by the carbonate of soda effervesced rapidly with muriatic acid, though previously it suffered no change; removing in this way the carbonate of lime, the residue was again treated with carbonate of soda, and by the second operation, furnished 8 parts more of crystallized oxalic acid, and still oxalate of lime was left undecomposed.

One hundred parts of the pulverised lichen, with 33 parts of sulphuric acid, and a sufficient quantity of water, were boiled for half an hour, filtered to separate the sulphate of lime, and the liquid collected very carefully evaporated, to avoid re-action of the excess of sulphuric acid on the oxalic acid. When cold, the whole became crystalline, and by pressure in bibulous paper, 17 parts of yellowish-white oxalic acid were obtained, some still remaining dissolved.

About the same quantity of oxalate of lime was found in the following lichens, *pertusaria communis*, *urceolaria scruposa*, *isidium corallinum*, *patellaria tartarea*,—*ventosa-rubra*,—*hematomma*, *bæomyces ericetorum*—*squamaria lentigera*, *placodium radiosum*,—*ochroleucum*, *psora candida*. The oxalate of lime is to these and analogous cryptogamous plants what carbonate of lime is to corallines, and phosphate of lime to the bony structure of more perfect animals. The oxalate of lime diminishes progressively in the family of lichens, as the species lose their crustaceous granular contexture, and acquire a foliated membranaceous or cartilaginous aspect; but the latter always retain a remarkable quantity.

Hence it will appear that the crustaceous lichens are an abundant source of oxalic acid, from which it may be obtained at low price. The common *variolaire* is found on almost all old and feeble beech trees, in the bark of which it is implanted, forming large white rough crusts, of variable thickness, which may easily be scraped off.

It will not be observed without interest, observes M. Braconnot, that oxalate of lime constitutes nearly one half of the weight of a number of organized beings, performing an important part in the economy of nature. It appears that by their means vegetation has commenced on the surface of the earth, since they are found

incrusting the hardest rocks, and the most compact marbles, wearing them down if left undisturbed. M. de Saussure met with them on the highest summit of Mont Blanc. M. Humboldt in his *Tableau de la Nature* says "these are the lichens by which the earth, void of vegetation in the north of Peru begins to be covered, *bæomyces roseus,—rangiferinus, lecide muscorum-icmadophylla*. Some other cryptogamous plants are joined with them in preparing for the vegetation of herbs and plants. Between the tropics, where mosses and lichens do not grow abundantly, except in shady places, some succulent plants, as the *sesuvium* or the *portulacaria*, supply the place of the earthy lichens.—*Ann. de Chimie.*, xxviii. 318.

26. *Composition, &c. of Formic Acid.*—Dobereiner ascertained a long while ago that formic acid, or a formiate, when put into from 6 to 10 times its weight of concentrated sulphuric acid, was resolved into carbonic oxide and water. He now finds the proportion to be 23.3 water and 75.7 carbonic oxide. Hence it may be regarded as constituted of 1 volume of the vapour of water 2 volumes of carbonic oxide gas, or

Carbon . . .	2 proportional . . .	12
Oxygen . . .	3 ——— . . .	24
Hydrogen . .	1 ——— . . .	1
		37

Ann. Phil. N.S. ix. 390.

Some time since, I had occasion to observe that the salts formed from the acid of ether, as described by Mr. Daniell, were resolved by sulphuric acid into carbonic oxide and water, precisely in the manner of M. Dobereiner's formiates. M. F.

27. *On the Fermentation of Sugar*, by M. Colin.—A memoir on this important subject is contained in the *Annales de Chimie* xxviii. 128, by M. Colin, who, in it, has described many new and important results relative to the theory of the action of ferments on sugar. Passing over his historical observations, we proceed at once to his experiments.

1. Fine wheat flour made into a paste without leaven, acquired in 36 hours a sweet savour; being left for some days it became sour, and then served as leaven to recent paste; the latter quickly becoming sour, was employed to convert 100 grammes of sugar dissolved in 400 of water into alcohol. The latter change was effected in one month.

2. Experimenting on gluten, it was found that 100 of sugar in 400 of water, mixed with 60 of fresh and well-washed gluten, became converted into alcohol in one month. The experiment

was made during the heat of summer; and it was effected in half the time, when the gluten, being weighed when fresh, was left to itself for 8 days till decidedly putrescent, before being added to the sugar.

3. Fresh beef mixed with sugar and water, in the same proportions as the gluten, *&c.* above, effected the conversion of the sugar into alcohol in three weeks. The meat being separated by decantation, hand-pressed, and slightly washed, was used to alcoholize a second equal portion of sugar; fermentation occurred as before, but rather more slowly.

4. The white of an egg mixed with 500 water and 100 sugar, required more than two months before alcohol was formed, but ultimately it was produced.

5. Cheese *à la pie*, well drained, obtained from little less than a litre of milk, left to itself for three days, was diffused in a solution of 100 grammes of sugar in 400 of water, the fermentation took place in three weeks. Butter-milk in place of cheese, did not succeed so well, in consequence of the diluted state of the caseous matter in it.

6. A little less than a litre of urine from a healthy person, with 100 grammes of sugar, *&c.* caused fermentation, completed in one month. The experiment was repeated successfully.

7. Finally, 30 grains of isinglass in sufficiency of water, 100 grammes of sugar, and enough water to make up a litre, fermented; the experiment being finished in four months.

In all these trials carbonic acid was disengaged. They were all made during the heat of summer, except the last, for which a stove was employed. All gave alcohol by distillation; and, it may be added, that in all, the residues underwent spontaneously a second vinous fermentation; a proof that alcohol arrests fermentation, and that though boiling may suspend it, it does not destroy the cause. The latter point was established directly by two experiments, in one of which meat was the ferment; in the other soft cheese. When these fermentations were arrested by ebullition, and preserved in that state for 10 days, activity was restored to them by contact of air for a time about equal to that required in the first instance.

These experiments require temperatures of at least 25° C. (77° F.); that withisinglass required a heat as high as 35° or 40° (95° or 104° F.); they were generally terminated in two months, but some were longer, others not so long when the temperature was favourable. Being carefully repeated, nearly the same quantity of alcohol was obtained, whether yeast was used, or any of these substances; namely, albumen, new cheese, urine, albumen coagulated and putrescent, gliadine, and especially tartarized gluten, i. e., gluten mixed with cream of tartar, and tartarized albumen. It was not found that zimoma was more effec-

tual than gluten, and M. Colin does not regard it as identical with yeast, or competent to replace it in producing rapid fermentation.

Similar experiments were made with pure fibrine, serum, coagulum of blood, its colouring matter, and ozmazone, with similar results. Serum and fibrine produced fermentation but slowly; but when the colouring matter remained much more rapidly. When coagulum of blood had been pressed and washed twice in water, then put into water to form a solution of its colouring matter, that solution, added to the sugar, caused a still more rapid fermentation than any of the former substances. When fibrine was used, the mixture, distilled a month after the rest, gave alcohol; the residue, left to itself for six weeks in a proper temperature, gave as much alcohol as before by distillation. The same was the case in all the other instances, and especially when the substances did not act with much power.

Generally, tartarized albumen acts more powerfully than coagulated and putrescent albumen; this more than glairy albumen; and the latter more than coagulated albumen. Gliadine is more powerful than zimoma, tartarized gluten more than gluten, and putrescent gluten more than that which is fresh.

No treatment by acids or ammonia could increase the power of albumen, or obtain from it any thing more active than itself. Nor was any advantage gained by mixing the substances two or three together.

M. Colin then proceeds to reason on these results; his reasoning we shall condense as much as possible. He asks whether it is not evident that distinct animal substances may excite alcoholic fermentation in sugar? and whether the effect is not more rapid as they have attained a certain degree of dissolution? Also whether it may not be presumed that all azoted organic matter has the same power? and he proposes to ascertain whether all organic matter, even such as contains no nitrogen, will not, when in a state of putrescence, produce vinous fermentation. He then remarks, that a ferment may be regarded as a substance, the presence of which determines a rupture of equilibrium; and, as the quantity of leaven generally employed is but small, such rupture cannot be comprehended, if it is not the result of a force the effects of which are transmitted by making the molecules of the fermentable substance pass in succession into a particular state; as, for instance, an electric force.

The observations of Gay-Lussac are then quoted. 1st. That fermentation will not commence unless air or oxygen be present; 2d. That a galvanic current will supply the place of air and commence the action; and M. Colin seems inclined to believe that the bubble of air, which is sufficient to begin the action, developes

electricity, which commencing the action, is transmitted from particle to particle, and so continues it. Yeast or leaven, he thinks, acts without contact of air in consequence of having previously been put into this state, and communicating it to the mixture.

Conceiving the state of fermentation to be an electric state, it is easy to conceive how the pile may be active in exciting it. A portion of beer yeast was prepared, a part was mixed with sugar and water, as before, and exposed to the solar rays, their effect on the thermometer varying during the experiment from 18° to 40° (64°. 5 to 104° F.), no signs of fermentation appeared in 12 days; in two months it was still very sweet, had began to mould, but contained nothing alcoholic or ethereal. Another similar portion, before being exposed in the same circumstances, was submitted for some hours to the action of a voltaic pile; it gradually entered into active fermentation, and at the end of 15 days the action was complete; all the sugar had given place to alcohol, and the taste was not acid.

M. Colin remarks, with caution, that it is possible all the different substances used may originate one constant substance, which is the true ferment; but he states his opinion, that such an effect is very little probable.

All the deposits formed during these fermentations were found effectual in causing the fermentation of fresh sugar; but the deposit from glairy albumen is the only one which, for the rapidity of its action, merits the name of a ferment.

In resuming; many animal matters transform sugar into alcohol, and with more facility, as they have been better prepared by a convenient putrefaction. From this reaction result deposits, which act upon sugar in a more marked manner; that from albumen acts like a true ferment, whilst albumen itself acts but very slowly; ferments or leavens then are formed during fermentation. Electricity acts a part in the change; it re-establishes the activity of a leaven rendered inert; but all developement of electricity is not proper for this purpose. Alcohol, as it is formed, prevents the fermentation: if ebullition suspends it, it does not destroy the cause. Finally, purified cream of tartar favours tardy fermentation, rendering the alcoholization more complete and rapid.

The property of converting sugar into alcohol has, therefore, been too exclusively attributed to yeast and sugared fruit, and it must be admitted at present, that the phenomenon of fermentation includes a greater number of facts than was supposed.—*Ann. de Chim.* xxviii. 128.

28. *On a destructible Green Matter, the Produce of a Mineral Water,*

by M. Vauquelin.—This substance was collected from the surface of the mineral water of Vichy, from a spring called l'Hôpital, by M. D'Arcet, and sent to M. Vauquelin, who examined it experimentally. It was contained in a bottle, in a state partly liquid and partly solid. The fluid part, first examined, was green by refracted light, red by reflected light: paper dipped into it, became tinged green, which, by slowly drying became clear blue. Caustic alkali appeared to destroy the colour of the paper, but dilute nitric acid, after a certain time, fully restored it, and after a longer time reddened it. It was not sensibly alkaline. Acids caused coagulation, and the filter then separated bluish-green gelatinous flocculi, a brown fluid passing. The flocculi formed a reddish-purple solution in carbonate of potash. When a few drops of nitric acid were added to this alkaline solution, a green precipitate becoming fine blue on slight excess of acid was formed. The flocculi were of an animal nature, and affected by heat like albumen.

Alcohol with the liquid separates green flocculi and a brown saline animalized matter remains in solution. Chlorine and nitric acid destroy the green colour and produce a red. Thus acids and alkalies act upon this colouring matter in a contrary manner to what they do on common vegetable colouring matter.

A portion of the liquid, to which nitric acid had been added and then evaporated to a syrup, was coagulated by alcohol; the deposited matter gave ammonia when decomposed by heat; the solution was found to yield crystals of nitrate of soda. A heat of 65° R. (178° F.) coagulated the green liquor; the aqueous part separated, was found by tests of silver and lead to contain a minute quantity of sulphur.

When the liquid was evaporated, brown pellicles formed on its surface of an albuminous appearance. When much concentrated it became decidedly acid and had a sugary taste. As, when evaporated and calcined, it yields carbonate of soda, the acidity must be due to some organic acid. Notwithstanding the pellicles deposited, much animal matter remained in solution, conferring the property of precipitating abundantly by infusion of galls.

By further experiments, it was found that the acid was the acetic, and both acetate of soda and lime were separated. These, though found in considerable quantity had not existed originally in the water, but had been formed in the bottle from the production of acetic acid, and its combination with the bases present. Ammonia was searched for but none found.

The green colouring matter is evidently very fugaceous, inasmuch as the heat of 212° destroys it. Whilst dissolved in the mineral water it probably has no colour, the water of Vichy being colourless. M. Vauquelin thinks that the circumstance of

its being found in such quantity in the water, being itself an insoluble substance, may be accounted for by the presence of carbonate of soda.

The *solid matter*, when filtered, washed, and drained on paper, was a brown viscid adhesive substance. Put into caustic potash or carbonate of soda it dissolved in part, leaving a pulverulent substance. When quite dry the substance was a yellow-green powder, which, by heat, gave crystallized carbonate of ammonia, empyreumatic oil, a little water and gas, leaving a black coal of the original form. In this way four parts became 3.34 parts, and then, by exposure to heat and air, became 2.62 of a reddish substance. Hence it contained about $\frac{1}{3}$ its weight of organic matter. The residue contained a few grains of sand with lime, oxide of iron and alumine, in the following proportions, 128,31 ; and 1.

After putting questions as to the original existence of this substance in the earth, or its being formed by chemical powers, *&c. &c.*, M. Vauquelin remarks that it is composed of three varieties of matter, one blue, coagulated by heat, acids, *&c.*; the other yellow, dissolving in boiling water, precipitated by alcohol and infusion of galls ; the third not precipitated by heat, acids, or alcohol, but by the astringent principle, these being probably different states of the same original principle. It evidently approaches more in its nature to albumen, than to any other principle known.—*Ann. de Chim.* xxviii. 98.

III. Natural History.

1. *Natural Transference of Rocks and Stones.*—Some particular phenomena of moving rocks in Carolina, have lately been noticed in the American Journals, and other publications, of apparently such a singular description as, in the first instance, to fail of obtaining credence; but being found to occur in more than one place, have now strongly excited the ingenuity of observers to explain them. Dr. Dwight relates in his travels that, being induced by the credited report of sober men, to examine an instance of this kind, he was taken to a lake, on the shore of which lay a rock which, though now two feet above the water, was declared by a person long resident on the spot, to have been at least two feet below the surface forty years ago, and 15 or 20 rods farther from the causeway on which they were standing. From the trees, stumps and other appearances on the causeway, it was evident the surface of the water and the shore had remained unaltered ; but upon examining the rock which was standing in water scarcely knee deep, a channel was found behind it towards the deeper water, formed in the earth, about fifteen rods in length, serpentine in its form, and sunk from two to three feet below the common level of the bottom on its borders ; in the front of the rock, the

earth was pushed up in a heap, so as to rise above the water, declining at the distance of a few inches obliquely and rapidly. A little way off was a smaller rock, exhibiting similar phenomena.

Another instance is referred to by Dr. Dwight described in the Collections of the Massachusetts' Society. It is stated that, at the bottom of a cove in the long pond in Bridgeton, are stones of various sizes, which, it is evident from visible circumstances, have an annual motion towards the shore, the proofs being that marks or tracks are left behind them, and bodies of clay driven up before them. Some, perhaps two or three tons in weight, have left a track several rods behind them, having at least a common cart-load of clay before them. These stones are many of them covered with water at all seasons of the year. The shore of the cove is lined with these stones three feet deep, which it would seem have crawled out of the water.

The Rev. J. Adams, who endeavours to explain these phenomena, states that there is a pond in Rhode Island, where similar phenomena are seen. He considers the expansive power of ice as fully sufficient to account for the effects observed; explaining them by supposing that the ice which, at an early period of its formation, had become fast attached to the stones, had then, as in its continued formation it expanded from the middle outwards, thrust them towards the shore. The expansion from the middle outwards he states to be well known to those acquainted with cold climates, and have observed the formation of ice; and mentions, that in large ponds and lakes, where thick ice has been formed, he has observed a disruption just at the edge, between the main body of ice and the shore take place, and the ice has projected on the shore a considerable distance over the line of disruption. Repetition of this effort, it is considered, would easily account for the motions of the stones.

In confirmation of this effect he remarks, that in New England, fences which originally stood erect near the edge of grounds, covered by water during the winter, have considerably inclined towards the shore as soon as the ice was formed, such fences always requiring to be placed upright in the spring. It is well known also among the farmers of New England, that if a stone fence be erected in a similar situation, it will after some time be overturned.

With reference to the cove at Bridgeton being lined with stones, which had apparently crawled out of the water, he remarks that many ponds and lakes in New England are lined with rocks in the same manner. While bathing on such shores he has frequently found a gravel bottom quite free from stones, until he had advanced to the depth of about three feet, when suddenly the bottom was covered with stones as far as he could reach it, in size and other respects, like those with which the shore was lined.

The explanation offered is, that the ice had removed these stones to a depth in the lake equal to its thickness, and had by degrees thrust them ashore.

Another observer, Mr. Wood, accounts for the effect also by the ice, but in a different manner. He thinks the ice may attach itself to all those stones which are near to, or project above the surface of the water, and thus retain them in one mass with itself; that upon the breaking up of winter the ice naturally thaws, and separates first at the edges of the ponds or lakes; that as water flows into these natural reservoirs from the dissolution of snow or ice, and the abundant rains of spring, it increases its quantity, and, by buoying up the ice and agitation, ultimately loosens many of the stones, to which the ice is attached, from the earth; that they are then borne about as with a floating island, are forced by the wind with the ice to the shore, and as the ice melts are dropped in succession much nearer to the shore than they were when taken up. Many of the tracks in the clay or mud may be formed by those stones which, projecting from the ice beneath, partially rest on the mud, but are still attached to the floating mass, and subject to its motions. The buoyancy of ice is such as abundantly to enable it to support stones even of a very large size.—*Silliman's Jour.* ix. 136.

2. *Distance to which Sand and minutely-divided matter may be carried by Wind.*—The following is part of a letter by Mr. Forbes, of Chapel-street, Tottenham-court-road: "On the morning of the 19th of January last, being on board the Clyde East Indiaman, bound to London, in lat. 16° 40′ N., long. 27° 41′ W., and consequently about 600 miles from the coast of Africa, at day-light we were surprised to find our sails covered with sand of a brownish colour, the particles of which, when examined by a microscope, appeared extremely minute. At 2 P.M. of the same day, having had occasion to unbend some of our sails, clouds of dust escaped from them on their being struck against the mast by the wind. During the preceding night the wind blew fresh from N.E.b.E., and of course the nearest land to windward was that part of the coast of Africa which lies between the Gambia river and Cape de Verd."

Mr. Forbes naturally suggests whether many of the seeds of those plants found in remote and new-formed islands of the ocean may not have been conveyed in the same manner.—*Europ. Mag.* 1825, p. 223.

3. *Rocking-stone, Savoy, Massachusetts.*—*Remarkable Limestone Rock.*—The rocking-stone is of granite, and venerable with the mosses and lichens common in this part of the country. It may be moved with ease, so as to describe an arc of about five inches, by the hands, or a shoulder, or by standing on its summit and leaning

the weight of the body on one foot and the other alternately. When the ground around it was first cleared, it was moved by the wind, and very probably this may be the case at present, though it is supposed to weigh 10 or 12 tons. The noise that it makes in moving is so little as to be scarcely noticed. The rock on which it lies is a coarse-grained granite, curiously contorted, and apparently stratified, the strata leaning to the west at an angle of about 45°. The rocking-stone lies on the very summit of this ledge and appears to touch it in three points, nearly in a right line across the strata.

By the sketch which accompanies the account, the forms of the stone resemble a low cone, with a convex base on which it rests.

Dr. J. Porter, who describes the above, also mentions a rock singularly posited in the S.W. part of Lanesborough. It is of limestone, and lies on another rock of the same kind. It is about 26 feet in length and 18 in breadth, touching the rock on which it lies, for about $2\frac{1}{2}$ feet, having no support at either end, and appearing ready to slide off and crush the observer. To the eye it has every appearance of a most magnificent rocking-stone, but it is immoveable. It is in the woods about $1\frac{1}{2}$ miles from Pitsfield village, and is beautifully and romantically shaded.—*Silliman's Jour.* ix. 27.

4. *Remarkable Oolitic formation of Saratoga County, New York.* Dr. John H. Steel, after describing the circumstances leading to the discovery of this formation, its particular locality, and its characters, states, " that in and near the road which leads from Greenfield to Ballston Spa by the way of Rowland's Mills, on the farm of Deacon Wood, there is a bank composed of a series of horizontal strata, where the peculiar characteristic features of this formation are well defined, and may readily be examined. One of the strata which compose the series at this place presents a very singular appearance, and one which if it occurs elsewhere, has never been noticed so far as I am able to learn by any writer. The surface of this stratum is fairly exposed, for a number of rods, both to the north and south of the bank beneath which it evidently passes; it is about two feet in thickness, and has imbedded throughout its substance great quantities of calcareous concretions of a most singular structure; they are mostly hemispherical, but many of them are globular, and vary in size from half an inch to that of two feet in diameter. They are obviously composed of a series of successive layers nearly parallel and perfectly concentric; these layers have a compact texture, are of a dark blue or nearly black colour, and are united by intervening layers of a lighter coloured calcareous substance, either stalactitical or granular; they are very thin, and I have counted

more than a hundred in one series. By breaking the matrix in which they are imbedded, they drop out entire, and may be readily reduced to any smaller size; by merely throwing them upon the rock the concentric layers easily separate, leaving the form exactly the same.

These concretions seem to be confined solely to one stratum of the series, and this stratum evidently accompanies the oolite in its whole extent, and is undoubtedly a variety of the same series, the best characterized oolite lying beneath, while those of a less definite character are regularly piled above it.—*Silliman's Jour.* ix. 17.

5. *Native Gold of North Carolina.*—Professor Olmsted gives the following account of large pieces of native gold, whilst speaking of this auriferous district. " Large pieces of gold are found in this region, although their occurrence is somewhat rare. Masses, weighing four, five, and six hundred pennyweights, are occasionally met with; and one mass was found that weighed in its crude state 28lbs. avoirdupois. This was dug up by a negro at Reid's mine, within a few inches of the surface of the ground. Marvellous stories are told respecting this rich mass, as that it had been seen by gold hunters at night, reflecting a brilliant light, *&c. &c.*, but no unusual circumstances were connected with its discovery, except its being nearer the surface than common. It was melted down and cast into bars soon after it was found. The spot where it occurred has been since subjected to the severest scrutiny, but without any similar harvest. Another mass weighing 600 pennyweights was found on the surface of a ploughed field, in the vicinity of the Yadkin, twenty miles or more north of Reid's mine.

" Mr. Reid found a mass of quartz, having a projecting point of gold of the size of a large pin's head. On breaking it open a brilliant display of green and yellow colours was presented, exceedingly beautiful. The gold weighed 12 pennyweights." " Although fragments of green-stone and of several argillaceous minerals occur among the gravel of the gold stratum, yet in the opinion of the miners, the precious metal is never found attached to any other mineral than quartz. It is rarely attached to any substance, but is commonly scattered promiscuously among the gravel." The gold country of this district is not less than 1000 square miles, and is situated between the 35th and 36th degree of north latitude, and between the 80th and 81st degrees of west longitude from London.—*Silliman's Jour.* ix. 5.

6. *On Ground Ice, or the Ice of Running Water.*—This subject has been taken up in a series of remarks by Professor Merian, who has read a paper on it to the Society of Natural History,

of Basle, from which the following abstract has been made. Ground ice is a name given to those detached and separated masses which running waters carry on their surface during a frost of some continuance. This ice differs from that which forms continuously along the banks of rivers, and particularly in places where the water is tranquil. It never forms on lakes, ponds, or stagnant waters, and motion appears essential to its production. It appears to resemble a floating aggregation of snow penetrated by water rather than common ice, but a more attentive examination shews that it possesses peculiar characters. It is, in fact, formed of an immense assemblage of small round discs of thin ice, similar to each other, and each some lines in diameter; each disc is perfectly transparent, though when conglomerated, the whole presents the appearance of a semi-transparent mass of wet snow.

It is well known, that before this ice will appear on streams and rivers, the temperature of the atmosphere must have been retained for some time several degrees below 32°, and it is generally observed that a cold wind blowing in an opposite direction to the course of the stream singularly favours the formation of this kind of ice, which, for this reason, will, in rivers of the same country, appear first on that flowing against the wind.

Although it may be supposed that in consequence of the superior levity of water at 32° above that which is a little warmer, the particular ice in question is formed at the surface of the water, yet it is to be remembered, that reasoning which applies correctly to standing waters, does not apply to rapid streams; and, as the name announces, ground ice really forms on the earth constituting the bed of the river, and there are few watermen or fishermen who cannot cite numerous cases of which they were eye-witnesses, having seen masses rise from the bottom to the surface, even from rivers of considerable depth.

In the winter of 1823, the canal of St. Alban, which conducts the waters of the river to Basle, bore a considerable quantity of ground ice. The transparency of the water was such as to allow of objects being seen three feet in depth. The bed of the canal at that part is covered by round pebbles. Wherever at the bottom of the water a projection occurred, either in deep or shallow parts, morsels of ice, in bundles, could be discovered, which, at a distance, appeared as collections of cotton flocks. In many places, the bottom was almost covered by similar flocks, which separated from time to time, and came to the surface of the stream, here very rapid. These flocks, when taken from the bottom, had exactly the same appearance as the *ground ice* which floated in abundance on the surface, and were composed, like it, of small plates of ice, rounded and agglomerated, so that no doubt could exist as to the similarity of origin. The parti-

cular and uniform arrangement of the ice at the bottom of the water did not permit the supposition that it had been precipitated from the surface.

M. Merian then endeavours to explain the effect by considering the water, though cooled at the surface by atmospheric cold, so mixed by its running motion and by the wind, as to have an equable temperature throughout, notwithstanding the difference of specific gravity between water at 40° and 32°. Thus the temperature of the bottom and the surface are rendered the same, and in this state, he considers the prominent bodies fixed at the bottom as offering points of attachment for the ice to be formed upon much more advantageous than the agitated surface, and this, in addition to their influence as nuclei of crystallization, as sufficient to determine the formation of ice there. The water, therefore, becomes ice in these situations, and especially in those places where projection offers shelter from the impetuosity of the current. The continued motion of the water prevents the formation of large masses, just as saline solutions, when stirred, deposit small crystals, and in consequence, only agglomerations of small plates are formed. When of a certain size, the force of the current and their lightness, causes their separation, and rise to the surface, bearing up, frequently, portions of earth and small stones.

M. Merian then collects together and compares the evidence of preceding writers, at length, but finds nothing which is inexplicable, or in discordance with the circumstances under which this theory explains the formation of ground ice.—*Bib. Univ.*, xxviii. 125.

7. *Luminous Snow-storm on Lochawe.*—Towards the latter end of March, in 1813, a shower of snow fell on Lochawe, in Argyleshire, which alarmed or astonished those by whom it was witnessed, according as they were influenced by superstition or curiosity. Some gentlemen who had crossed the lake in the morning had a good opportunity of marking the phenomenon. All had been calmly beautiful during the day, and they were returning homewards from Ben Cruachan, when the sky becoming suddenly gloomy, they rowed more smartly towards the shore, in order to avoid the threatened storm. In a few minutes, however, they were overtaken by a shower of snow, and immediately after, the lake, which was of glassy smoothness, with their boat, clothes, and all around, presented a luminous surface, forming one huge sheet of fire. Nor were the exposed parts of their bodies singular in this respect, for, to the eye, they all seemed to burn, although without any feeling of warmth. When they applied their hands to any of the melting snow, the luminous substance adhered to them, as well as the moisture; and this property was

not lost by the snow for twelve or fifteen minutes. The evening became again mild and calm, but lowering, and very dark. The natives had not witnessed any similar appearance before, and many of them believed it the forerunner of some dire calamity, that was to befal their mountain land.—Rev. Colin Smith. *Edin. Phil. Jour.*, xii. 405.

8. *Effects of Lightning; lateral discharge.*—Dr. Fusinieri relates, that being in an upper story of his house, at Vicenza, at a time when lightning fell 400 or 500 yards off, there appeared to him in the room at the moment certain luminous flocculi, of various dimensions, making a cracking noise, like electric scintillations, proceeding from a window in the direction of a diagonal line, drawn from a steeple struck by lightning. The scintillations were very distinct in the universal gloom which existed, and the cracking noise sufficiently convinced him that the effect was not an optical illusion. Dr. Fusinieri thinks the fact proves that a torrent of electricity radiates, not merely light, but electricity itself, and considers the effect as tending to explain, more distinctly than heretofore, what has been called the *lateral* discharge of lightning.

The lightning fell on the church of San Giuliano, and there affected some persons. One Sig. Toniello, whose left arm was bare, was struck by it. Upon recovering from the state of insensibility into which he was thrown, it was found that the naked arm had five wounds, like cuts, attended with effusion of blood; and it was three months before the limb was well again. It is to be remarked, that the same arm was injured three years previously, by some fire-works.

For five months, nothing particular was observed in the arm, but on May 2, 1824, a thunder-storm occurred, when the use of the arm was suddenly lost, and a sensation of great heat experienced from the hand to the elbow. In two or three days, these effects disappeared. On the 15th of May, similar weather returned, and the same sensation of heat; the arm feeling on fire, and allowing of no rest at night. The day following, the heat had ceased, the arm felt in pain and fatigued, but in three days, all was well again. Finally, whenever, during the year, any return of similar weather took place, the same effects were observed, namely, diminished motion in the arm, sensation of great heat, and a sense of heavy obtuse pain and fatigue, principally where the five wounds had existed.

It was found that the sensation of great heat was not accompanied by any actual elevation of temperature in the part, the temperature of the whole arm being uniform. It was found, also, that no effect of this kind could be produced by vicinity of the arm to a charged conductor of an electrical machine, or

even by small sparks taken on or near the part: on the contrary, sparks taken into the extremity or knuckles of the fingers, produced good results; a few scintillations, even, dissipated almost entirely the sensation of heat, and restored the use of the arm. Once, indeed, a slight shock from a Leyden jar brought the hand and fingers into such a state, that being curved, they could not be opened; but slight scintillations in a few minutes restored it. No permanent good was, however, at any time afforded by the electrical machine, the arm constantly becoming affected by any similar state of the atmosphere. Whether the effect will continue during another thunder season, is anxiously looked for by M. Fusinieri.—*Giornale di Fisica*, vii. 284.

9. *Singular imperfection in Vision.*—A man about thirty years of age, of a robust constitution, in perfect health, suffering no inconvenience in the head or organ of vision, having good vision, eyes well formed, and in nothing apparently different from what they ought to be, has not the power of distinguishing colours. From the time that he first began to reflect on the appearance of things about him, he perceived that bodies made various impressions on his eyes, according to their colour; nevertheless he did not experience that sensation by which colours are estimated and distinguished with precision, and in this respect could not distinguish accurately the bodies to which they belonged. The various colours appear to him more like gradations in the illumination of the objects around him, than like tints dependent upon diverse refrangibility. This defect has not undergone any change by his advance in age. Amongst the various colours, pale yellow, scarlet, and blue, are perhaps those upon which his judgment is most certain; but green, faint red, strong yellow, and brown, cause the greatest confusion The same colour seen alone, but at different times, always produces the same sensation; in this he never errs; but when it is presented to him with other colours, he is at fault.—*Giornale di Fisica*, vii. 470.

10. *Insensibility of the Retina.*—M. Majendie has stated to the Academy of Sciences, that he has ascertained the insensibility of the retina, in the eye of a woman suffering from cataract. The contact of an instrument with that organ produced no appreciable sensation; the person recovered sight immediately after the operation.—*Ann. de Chim.* xxviii. 183.

11. *Preservation of Anatomical Preparations.*—Dr. Godman objects to the substances usually recommended for the preservation of anatomical preparations, such as saltpetre, common salt, corrosive sublimate, and pyroligneous acid, in consequence of their action upon the edges of the knives, the two best as preservers of